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LECTURES
ON
COMPARATIVE ANATOMY
AND
ANIMAL PHYSIOLOGY,

DELIVERED BY

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LECTURE XIX.

ON THE OSTEOLOGY OF BIRDS.

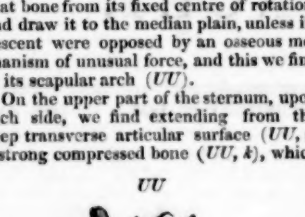
THE arms of birds are organised for the air, and their legs for the land or the water. No parts of animals vary so much in form and structure as the atlantal and the sacral extremities; the parts remotest from the centre of the skeleton are the most mutable in form; and the organs of progressive motion conform most to the medium in which animals reside. These parts vary indeed so much in the same class of animals, that we might almost be induced to imagine that in organs so different as the human hand and the fin of the porpoise, or the wing of the bat and the hand of the mole, all unity of composition was lost; and in passing to different classes we should scarcely expect to find the same elements of structure which compose the fin of a fish or of a turtle, metamorphosed into the wing of a bird. But these very diversities of form of the same organ, when carefully examined, present the best proofs of the unity and simplicity of the plan upon which all organic forms are constructed. The same instrument which serves the seal to cleave the depths of the sea in pursuit of its prey, and the mole to dig its subterranean labyrinths, carries the eagle through the regions of the air, and the migratory birds over mountains, and continents, and the ocean. No muscular mechanism could give this power to the quadruped; it must have every system, down to the generative, constructed on the plan of the birds. With twins in the uterus, filling the abdomen, and pressing up the viscera against the lungs, how could the eagle, spurning the dull earth, dart like a meteor through the air, and ride at ease above the clouds? How could it soar aloft like a balloon through the attenuated air if its respiration were confined to the lungs, and its carcass were weighed to the earth with a load of useless marrow, filling all the cavities of its bones? Its respiratory system, like that of an insect, passing through all parts of the system, through the bones of the face and the skull, through every vertebra of the column, and through almost every bone to the hands and feet, through the lungs, and through the large air-cells within and without the trunk, not only renders the body light and buoyant, but by aerating the branches and capillaries of the systemic as well as the pulmonic vessels, the temperature of the blood is raised, to develop the ovum by incubation, increased energy is given to the muscular system for their aerial exploits and their migrations over the globe, and redoubled activity is communicated to the circulating, the secreting, and all the other systems of their economy. Their nervous system and the organs of the senses run rapidly forwards to a high stage of development; ossification and anchylosis proceed through their skeleton with unparalleled activity, and to an extent unequalled in other animals; and the hairs on the surface of their body are metamorphosed into feathers, to serve as light elastic instruments to impel them through the air, so that every part of their economy conforms to their aerial life.

We have seen the extensive surface presented by the sternum for the attachment of the powerful muscles of the humerus, and the solidity of the trunk, by which the force of those muscles is re-

doubled. But muscles so powerful, and acting with such force, instead of rotating the humerus in its glenoid cavity, and bringing down the whole arm against the resisting air, would pull down the head of that bone from its fixed centre of rotation, and draw it to the median plain, unless its descent were opposed by an osseous mechanism of unusual force, and this we find in its scapular arch (*UU*).

On the upper part of the sternum, upon each side, we find extending from the deep transverse articular surface (*UU, i*) a strong compressed bone (*UU, k*), which

UU



rises upwards, backwards, and outwards, to the head of the humerus, where it presents a slight arched articular depression for the attachment of the head of that bone. This large and powerful bone we see continued backwards from the shoulder-joint, in the form of a flat curved bone, extending, parallel with the vertebral column, along the outer surface of the dorsal part of the ribs. The bone which thus passes down and expands at its upper and lower part, extending from the head of the humerus to the sternum, is the *coracoid* bone (*UU, k*). The narrow flat portion which extends on the back part of the trunk, in this curved form, along the side of the vertebral column, is the *scapula* (*UU, a a b*). We observe, thus, that the scapula is generally much less than half the size of the coracoid bone with which it is partially anchylosed. The great object of this scapular apparatus is to give attachment to the powerful muscles of flight, and to keep the heads of the humeri asunder during the violent effort to bring down the wings to strike the air, to prevent the powerful action of the pectoral muscles from causing the two heads of the humeri to approach each other on the median plane. The principal pillar of support and means of resistance is, obviously, the coracoid bone. Were that a slender or thin bone, it would yield to the powerful action of those enormous muscles, and would instantly be broken to pieces; and in place of the humerus rotating upon the articular surface (*UU, c*) at the end of the coracoid bone, it would be actually carried down towards the *entosternum* (*UU, g*) in the direction of the pectoral muscles. The head of the humerus would be carried down altogether from its fixed position (*UU, b c*). It is thus that the coracoid bone, by far the most important in these scapular elements, is the most largely developed, and that the scapular bone itself (*UU, a b*) is so remarkably small.

Higher or more anterior than the two coracoid bones, you observe an arch (*UU, d d e*) formed by the meeting and anchylosis of two bones. These two bones come down from the extremity of the coracoid bones, and the anterior extremity of the scapulæ, and they direct their course towards the extremity of the crest of the sternum; they anchylose together, however, and sometimes, as in this pelican, they are anchylosed to the entosternum (*UU, g*). They are the two *clavicles*. They are anchylosed together on the median line in birds, to form a solid arch, in order that they may give all the assistance which their strength will allow, to keep the head of the humerus on each side from coming down towards the median plane. From that being its use, you must at once perceive that in the rapacious birds, which have the most powerful flight, that bone, composed of the two anchylosed clavicles, must be most powerfully developed. Thus it is, that in the eagles, in the hawks, in the kites, in the condors, you observe not only that the bone is comparatively strong, but that its two parts meet at a very obtuse angle. I hold in my hand the two clavicles, anchylosed, of the great condor of the Andes, a bird which has been often seen with a stretch of wing of more than fifteen feet. Compare the bone which I hold in my hand of this great condor with that of our largest and most rapacious British bird, the golden eagle, and you see how remarkable is the contrast. This clavicular or fork-like bone is ten times as large as that of our golden eagle. In the birds, therefore, which are not destined to fly much through the air, you observe that that bone, the forked bone, composed of the two clavicles, is a slender bone, and that its two parts meet at an acute angle, as in this peacock (*UU, d d e*), and other gallinaceous birds.

The more slender the clavicles here, and the more acute the angle at which they meet, the less resistance they must obviously present to the approach of the humeri. In all these gallinaceous birds (*UU, d d*) you observe how long and slender these clavicles are, and the acute angle at which they meet. Their anchylosis on the median plane is an obvious advantage for the

purposes for which these bones are introduced; and they afford also an example of that anchylosis which unites so many elements, in their skeleton, of parts that are detached in other animals. Those bones are placed apart from each other at their median terminations, and have the sternum interposed between them in quadrupeds, as in man. In the ostrich they are of little use, and are very imperfectly developed. They only form two slender processes, extending downwards and inwards from the scapula; and they do not meet, nor nearly touch each other,—showing of what little importance they are in these heavy running birds, and how much they approach, in them, the detached form which they have in mammalia.

Thus we see that the common elements which exist in the scapular arch in the human body, have changed their forms, their magnitude, their proportion, to suit the living conditions of the bird, and the important uses which it is to make of those bones, without, however, changing their true nature and their connexions.

Looking at the wing of the bird in its entire state, covered with all its feathers, and moving in one piece, you would little expect to find so close a resemblance in its internal structure to the human arm, as you actually find upon examination. The *humerus* (*NN*, *e*) commences by an expanded broad head, which has a convex protuberant articular surface, which plays freely upon a very shallow arched depression, formed by the coracoid and the scapular bones. It is marked behind by a deep depression, at the bottom of which you perceive the apertures through which the air entered—the air that was conveyed through the air cells of the axilla from those of the lungs. By these apertures the air had admission to the rest of the bone, and displaced the thin serous marrow that originally, in the young state of the bird, filled all this bone. At its distal extremity we observe it curved, a little bent forwards, and terminating in two condyles, upon which chiefly plays the *ulna* (*NN*), the *radius* (*NN*) being but a slender bone. We observe in the radius and the ulna of birds, that the articulation of these two bones together is such, as to admit of almost no pronation and supination,—that the hand of the bird is fixed in a state of pronation, and that its motion at the wrist is in the reverse direction, or at right angles to the ordinary motion of the wrist of quadrupeds; for in place of possessing a power of flexion and extension, the motion at the wrist in birds is that of abduction and adduction, as we see also in the bats. That is the best possible direction of motion for the hand of birds. It is the best position to fix the hand of the bird in a state of pronation, and in such a manner, that the impulse given to the air shall be downwards and backwards, and shall have no possible means of bending back the hand, as that would put an end to all effective impulses given to the air. It strikes the air, therefore, with greater force, an advantage entirely resulting from the mechanism of the solid parts, which do not allow of their fingers or hand bending up by the stroke given to the air. But there is another obvious advantage in the birds possessing only the power of abduction and adduction, that in folding the wing the hand is placed along the dorsal margin of the ulna; that in the folded state of the wing, the little-finger of the bird, like that of the bat, runs along in the direction of the olecranon of the ulna, so that the whole wing when folded applies closely and compactly to the side of the trunk in birds, and the large quill feathers of the wing thus applied closely along the trunk, reach the tail of the bird. These long quill feathers covering the hands, would themselves effectually prevent the finger of the bird from folding in the manner of those of mammalia.

At the wrist in the bird we observe only two small compressed bones (*WW*, *c d*),

two bones of the carpus. These two bones are placed in such a way as to admit of extensive and easy adduction and abduction at the wrist, but so as to prevent flexion and extension, or any kind of rotation, which would here be a great imperfection in the mechanism of the hand.

The metacarpal bone (*WW*, *e f g*) is a single but an interesting bone; it has the rudiments of three separate bones anchylosed together. At its upper part or proximal end it is very broad, and it has a process extending forwards towards the extremity of the radius, which has the thumb bone or inner finger (*WW*, *h*) attached to it. Extending downwards, we find that the greater part of the metacarpal bone is composed of two pieces that are anchylosed together both above and below, and separate in the middle of their course. These two portions (*WW*, *f g*) towards the ulnar side of the hand, give attachment at their distal extremity to two fingers. You would therefore say, from the outward form of this aggregate metacarpal bone, which is anchylosed into one piece,

as well as from the connexions of the bone, and from the parts that are articulated to the bone, that it has the elements of three metacarpal bones united together, like the two bones of the wrist and ankle of *ruminantia*; one of those elements (*e*) in the birds serving for the thumb, the second or middle element (*f*) serving for the attachment of the middle, the largest finger, and the small lengthened portion (*g*) serving for the attachment of the little-finger.

The thumb (*WW, h*) consists generally of one bone, broad at its proximal end and tapering to its distal end. The middle finger of the bird consists generally of two remarkably compressed and lengthened bones (*WW, k l*), and a small terminal phalanx—three phalanges of the middle-finger. The little finger (*WW, i*) consists of a single compressed slender tapering bone, articulated to the outer or ulnar element of the metatarsal bone, and the smallest phalanx in this very simple, though lengthened hand of the bird.

Zoologists employ these regions of the arm to characterize by distinct names the feathers of the wing. They call *primary* feathers the feathers that are attached along the ulnar side of the hand. *Secondary* feathers are those attached to the forearm. The *scapulary* feathers, those that lie over the humerus, a term not very appropriate. The *bastard* feathers are those attached to the thumb.

We observe, however, that although those names of thumb, middle-finger, and little-finger, may, in a general way, be given to the three fingers of the bird's hand, they are in fact not analogous to the thumb, and the two succeeding fingers of the hand. That would be easiest ascertained by observing in some birds which have an additional finger, whether that finger is upon the radial or upon the ulnar side of the hand. Sometimes the thumb terminates in a spur. Sometimes there is a spur upon a distinct phalanx on the radial side of the thumb, showing that this inner finger (*WW, h*), which is generally in the position of a thumb in the bird's hand, is not the true analogue of the thumb or inner finger of the hand of mammalia. And when you reflect upon the imperfect development and gradual disappearance of the thumb in quadrupeds, the shortest member in the human hand, the first that is lost, as you descend through the mammalia, you would not expect to find it where there are so few fingers left on the hand of the bird; you would rather expect those more permanent fingers to be left on the hand of the bird, that are observed to be more permanent in quadrupeds. We shall see that the thumb is lost externally in the very first order of quadrupeds below man; that in many of the quadrumanous animals there is not even a wart projecting from the hand in the situation of the thumb; that there is, sometimes only, as you see in the spider monkey, four fingers on the hand; two compose, almost alone, the hand of the *ruminantia*, and those two fingers are anchylosed into one in the *solidungula*.

The pelvis (*XX*) of birds consists of the

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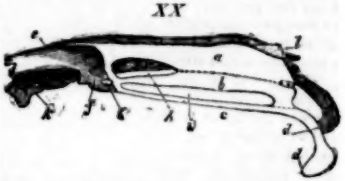

ordinary three bones, the *iliac* bones (*XX, a*), the two *ischia* (*XX, b*), and the *pubic* bones (*XX, c*). The iliac bones anchylose to the transverse processes of numerous sacral vertebræ, so that the sacro-iliac articulation is of great extent and security. The iliac bones anchylose with the ischia; and the sacro-sciatic notch of mammalia is here a foramen (*XX, h*), except in the ostrich. The ischia anchylose at the cotyloid cavity (*XX, f*) with the pubic bones, so that all these elements of the pelvis anchylose together, and they unite to form the acetabulum for the femur, as in most other vertebrated animals. These bones are dense, compact, and strong, on the upper and dorsal part of the pelvis, admitting air into all their cavities. They are light, compact, white, and dense, in their texture, like the other bones of the skeleton, where air is freely admitted into their interior cavities. They all taper downwards and backwards, and acquire greater elasticity as they approach to the posterior and lower part of the pelvis; and the pubic bones (*XX, d d*) have their anterior terminations at a distance from each other. This is the normal character of these parts in this highest of all the oviparous classes.

We observe that, most generally, the *sacro-ischiatic* notch of the human skeleton forms a distinct foramen (*XX, h*) in birds, by the anchylosis of the ischium with the ilium and sacrum. The *obturator* foramen (*XX, i*) has here a remarkably lengthened form, corresponding with the lengthened form of all the bones of the pelvis. The pubic bones of birds are free at their terminations, and elastic, in order that they may give the necessary support to the contents of the pelvis, and at the same time be susceptible of dilatation when the egg is passing out

from the cloaca. They thus give both support and the means of egress to the contents of the pelvis, by their being thin elastic bones, and open at the symphysis of the pubis. Had they been anchylosed in front, or had they met by a symphysis which would not admit of dilatation, the egg of the bird would necessarily be broken, because the egg of the bird acquires all the solidity which it is ever to possess, within the cloaca; and, therefore, not having the means of yielding to pressure, like the imperfectly ossified skulls of mammalia, and the soft membranous eggs of reptiles, it has the means of free and safe egress by the pubic bones being permanently detached—free at their extremities, and admitting of dilatation.

The pelvis of the ostrich forms a remarkable exception (as indeed that bird does in many other parts of its structure) to the ordinary and normal character of the skeleton in this class. In the ostrich we observe nothing remarkable in the forms of the iliac bones; but we observe that between the ischium and the ilium the sacro-ischiatic notch remains a notch, as in the pelvis of quadrupeds, and is thus contrary to the normal character in birds. The obturator foramen remains a foramen of a lengthened form, as in other birds.

The pubic bones, however, you will observe in this large pelvis of the ostrich, have not only met and firmly united, but have actually anchylosed at their symphysis. This is a structure of the pelvis which is anormal in the ostrich, and brings it, in that part of its body (as in almost all its other peculiarities), very near to the structure of quadrupeds. Even the form of the ischium on each side approaches to the rounded form of the ischium, so common in the mammalia.

The form of the symphysis in the ostrich even approaches to the broad form of those two terminations of the pubic bones in quadrupeds. The cotyloid cavity is formed by the junction of the three ordinary elements of the pelvis, situate far forwards in the ostrich, that the hind legs may poise the great carcass of that bird as nearly as possible under the centre of gravity. These enormous and strong bones of the pelvis of the ostrich give attachment to powerful muscles which move the legs of that bird, which you observe to have bones of the leg like the bones of an ox's leg,—a muscular strength by which, though they cannot fly, they are enabled to outstrip, by running, the fleetest race-horse, when pursued over the burning sands of Africa. So that although it has not wings capable of raising it into the air, it has the means of outstripping and escaping from all its ordinary foes. Its wings are but imperfectly constructed for flight, from the feathers having their barbs soft and decomposed. The barbs are not here stiff and fixed to each other, but are soft, downy, and float in that loose and elegant manner which is familiar to you, and which prevents the wing from giving any forcible or effective impulse to the air; the air entirely escapes between the detached fibres or barbules of the feather. It is from that decomposed and soft downy character and elegant form of the wing-feathers of the ostrich, that they are so valuable in commerce as ornamental articles of dress. They are valuable organs to the ostrich, although not for flying; for they form a spreading fan to cool its heated carcass after its rapid course, scorched by the sun above, and reflected by the scorching sands of the desert. Every organ has its use in the living economy of animals, although their peculiar or reduced forms, or our ignorance of the living habits of the species, often conceal their uses from our view.

Although the anterior part of the pelvis in birds is open and remarkably feeble and yielding, the whole dorsal portion, where the cotyloid cavity is placed for the reception of the strong bones of the legs, possesses great strength—the part which is most pressed upon by the os femoris, while the whole trunk of the bird rests solely upon the hind-legs. It acquires this strength, not only by the great development of the iliac bones, and by the strength and great magnitude of the sacrum, but also by the complete anchylosis, as well as the remarkable extension, of those bones composing the back part of the pelvis. The vertebral canal is enlarged in the middle of the sacrum between the two cotyloid cavities, like a cranial cavity, in birds, at the place where the great nerves of the legs pass into the spinal marrow, and the intervertebral foramina are distinctly double along each side of the whole sacrum, as you see in this of the wild swan (*XX*). The cotyloid cavity forms a complete foramen, its posterior or inner part not being filled with bone, but only by membrane, as is general in the partition of the orbits.

The head of the femur (*NN, v*) in birds is very short, small, and rounded, with a very short cervix, and projects at a right angle a little lower than the trochanter major, which here forms an extensive arch from before backwards. The femur is almost always a short and strong bone, hollow throughout internally, and with thin, compact, diaphonous parietes, whatever be the length of the legs: so that the length of the legs, which is so remarkable

in the grallatores or the wading birds, and so short in the swimming or aquatic birds —those differences in length do not depend upon the femur, but upon the bones of the leg that succeed. The air is admitted into the femur by a large aperture on the fore part of the trochanter major.

We observe between the two prominent sharp condyles of the lower part of the femur, the patella, situate as in mammalia. The upper part of the long *tibia* (*NN*, *w*) receives the lower extremity of the femur, and derives little aid in that articulation from the imperfectly developed *fibula*. The fibula is always, in birds, an imperfectly developed bone, generally small, tapering and anchylosed at its lower part to the tibia, although separate at its upper or proximal part. Sometimes it is detached throughout the whole length. It tapers, you observe, downwards, to a fine spiculum of the bone, towards the middle of the tibia, and there imperceptibly terminates; whereas the lower part of the tibia is very much expanded, and strong, for a broad and secure articulation with the tarsus. We observe in this disappearance of the fibula at its lower part, an illustration of a law that is very constant with regard to the extremities of animals. The central part of the skeleton is the vertebral column. The parts that are most remote from the centre of the vertebral column, are those which undergo the greatest number of changes of form. We observe that the bones of the extremities become anchylosed, both in the anterior and posterior extremities, and that they disappear, not from their nearest parts, but from their distal towards their proximal extremity, so that in the arm we observe the *ulna* first becoming anchylosed at its lower end to the radius; then it begins to be diminished,—to become gradually more and more undistinguishable, from the lower part of the ulna upwards towards the olecranon, until we find only the olecranon left, anchylosed to the upper and back part of the radius; or left loose, to form a rotula of the elbow, as in some bats. So it is with regard to the *fibula*; its distal extremity disappears first, and it gradually disappears from below, up towards its proximal extremity, as here in the leg of the birds.

The tibia in birds is a bone which varies remarkably, both in its length and in its strength. Where the birds wade on the banks of lakes and rivers, or of the sea, in search of the young fishes that come in myriads towards the shores, we find that that part of the leg below the knee is very much lengthened, and that it is totally destitute of plumage, so that it forms the appearance, when the bird is walking in the water, of long naked stilts. It is from this appearance that those wading birds have received the name of grallatores, as you see in the storks, in the cranes, the herons, the spoonbills, and many others. In these you observe the tibia to be of great length, and the *metatarsal* bone (*YY*, *a b*), which succeeds, to have also a very lengthened form. That metatarsal bone in birds

somewhat resembles the long common bone of the legs of ruminating and solidungulous quadrupeds, which succeeds the tibia and the small bones of their tarsus. It has an irregular or uneven proximal articular surface, with two deep depressions, one on each side, for the two prominent condyles at the lower extremity of the tibia. The metatarsal bone presents at its lower extremity, generally, three articular pulley-like processes, serving for the attachment of the three toes which you most frequently find in birds directed forwards. It contains the rudiments, in fact, of three metatarsal bones anchylosed into one, as you observe by its trichotomous division at its lower end. There are no tarsal bones in birds; and we observe that in passing downwards through the class mammalia, the small bones of the tarsus gradually disappear, until we are left with very few in the ruminating animals. This pulley-like form, which you observe at the lower part of the metatarsal bone in this domestic cock (*YY*, *b*), you find remarkably imitated in the corresponding metatarsal bone of the long leg of the *jerboa*, one of the *rodentia*. It terminates below, in three pulley-like articulations. Besides those three pulley-like terminal articulations, we can generally perceive at the lower part of the metatarsal bone in birds, at its inner and posterior surface, a depression which marks the attachment of an *inner metatarsal* bone (*YY*, *c*), a small bone—a bone that is connected with the thumb of the bird, or, rather, with the toe that is directed backwards, and which, consequently, is like a thumb in function, being opposable to the other toes. That posterior toe is attached to the metatarsus of the bird by a detached bone, a metatarsal small bone. The attachment of that small

bone leaves a depression upon the large metatarsal bone (*YY*, *a b*), so that even where you cannot find the metatarsal bone of the toe that is directed backwards, you will find that on the large metatarsal bone you have an indication of the former presence of the posterior toe in this articular depression of its metatarsal bone.

Thus we have generally in birds three toes directed forwards, and one toe directed backwards. The first phalanges of the toes directed forwards, have their proximal extremities articulated with the three pulley-like terminations of the great metatarsal bone. The outer toe of birds (*YY*, *d* 1), the one corresponding to the little-toe of the human foot, has five bones in it. The second toe (*YY*, *e* 2), which is the middle toe of those directed forwards, has four bones in it. The third toe (*YY*, *f* 3), or the innermost of the three toes that are directed forwards, has three bones in it. The fourth toe (*YY*, *g* 4), or the one that is directed backwards, when it is present, has two bones. It is remarkable to observe the constancy of this arrangement of the bones,—this numerical relation of the osseous parts of the feet of birds.

Zygodactylous birds, woodpeckers, cockatoos, parrots, and such-like climbing birds, have two toes directed forwards, and two directed backwards, as you see in the cuckoos and woodpeckers before you; they have two toes directed backwards in consequence of the outer toe being reversed, turned backwards like the inner toe or thumb of the bird. That we can easily ascertain by counting the number of bones that are in the toes which are directed backwards. You could, by ascertaining that one of these toes had five bones and the other had two bones, discover that the two which are directed backwards, are not the two toes which are next to each other in the foot, but, on the contrary, the two which are most remote from each other, which are in all such birds directed backwards, and, therefore, that the two toes which in those cases are directed forwards, are the two middle toes, or the second and third from the outside of the foot of the bird.

Many birds have three toes, as you see in the cassowary, in the emu, in the American ostrich, and in many others. By counting the bones of the three toes that are left, you at once see whether the outer toes have disappeared or the inner toes. You find that the outermost of these three toes has five bones, that the middle toe of those birds has four bones, and that the inner toe has three; so that the toe that is lost is the one that in most birds is turned backwards.

The ostrich of Africa, however, has but two toes on each foot. One of those toes, the inner, is of great size and strength, and the other is a comparatively small toe without a claw, whereas the large toe is covered with a hoof almost like that of a ruminating animal. It is the great instrument of progressive motion, and that with which by its kicks it defends itself when pursued or irritated. You perceive, by examining these bones of the toes of the ostrich, that the outer small toe, notwithstanding its shortness, and being destitute of a claw, has, as in the outer toe of all other birds, five bones, and that the next toe, more than double the size of the outer, has, notwithstanding, only four bones. The ostrich, therefore, is deficient on the inner part of the foot, and wants nothing on the outer, excepting the claw. The most permanent part of the foot of animals is the outer. So is it in the hand of quadrupeds; the thumb is soon lost; and in the hand we observe that the next finger becomes diminutive, and so does the little finger in quadrupeds, until they are only left in an imperfect rudimentary state, and the two middle fingers only are continued to the ground, as in the ruminating animals, which are anchylosed into one in the zebra and other *pachyderma*.

I have examined often the very extensive collection of skeletons of birds preserved in the Museum of Comparative Anatomy at Paris, and have observed that this arrangement of the bones of the toes is remarkably uniform in the class. Exceptions have occasionally been mentioned in books, but those exceptions generally depend on the imperfect mode in which the legs have been articulated after the maceration of the bones. There is a skeleton of the ostrich in the museum at Paris, which has four bones in the one toe and four bones in the other. That was one, perhaps, of the earliest articulated specimens of the ostrich's feet in Paris. It was described in the *Lec. d'Anat. Comp.* by Baron Cuvier from this skeleton, as having four bones in each of the toes, naturally enough. A splendid figure of this foot of the ostrich—the bones of the foot—was published by Marischal, the distinguished draftsman, formerly employed in the Garden of Plants; and that large folio engraving gives four bones to the outer, and four to the inner, toe of the ostrich. A skeleton was afterwards articulated under the inspection of Baron Cuvier, in which attention was paid particularly to the osteology of the feet of the bird: the five phalanges of the outer toe were then found as in other birds, and that skeleton now stands beside the former erroneous one. Cuvier corrected his original error

in the *Regne Animal*. There is scarcely one correctly articulated skeleton or foot of the ostrich at present (1833) in the British dominions.

I have not met with any exception in the class of birds to the numerical relation I have mentioned of the phalanges of the toes. The exceptions stated by Carus I have carefully examined in the collections at Paris and elsewhere, and there are none of those mentioned by him that are really exceptions. The skeletons which have been carefully articulated, show that the bones pursue the same normal relation.

We see thus, that in the most frequent structure of the foot of birds there are but four toes, three directed forwards and one backwards; so that we have here lost one of the toes of the mammalia, one of the toes common in reptiles and even in amphibia. Which of the toes should we expect to be thus lost in the bird, in passing downwards, or indeed in passing upwards, for both above and below we find five toes developed? From the ordinary law of the deficiency, and the little use, comparatively, as a means of support, of the inner toe of animals, we should expect that that rather than the outer is the toe which is deficient in the bird. So that if that fifth member of the foot should be found in some species of birds, although deficient throughout the class generally, you would expect to find in that so generally deficient member not two bones, because there are two bones in the fourth toe that is directed backwards. We should expect to find in it one bone less than in the usual inner toe, which is directed backwards, and in many gallinaceous birds we observe the spur of the male (*VV*, 5) to be placed on the inner side of the long metatarsal bone, and to be supported only by a single claw-bone, similar to the last phalanx of all the other toes of the foot. The respective numbers of phalanges in the toes of birds, therefore, passing from without inwards, are 5, 4, 3, 2, 1. The inner toe of the foot of quadrupeds, the posterior thumb of quadrumana, and the great toe of the human foot, thus shrunk to the spur of a cock! And yet, to pursue this matter curiously, it is even so.

Thus the skeleton of birds, remarkable among the vertebrated classes for the uniformity of its structure throughout nearly five thousand species of the feathered tribes, and the first we have seen of the warm-blooded animals, presents characters intermediate between that of the cold-blooded reptiles and that of the highest class to which man belongs. Notwithstanding the extraordinary effects on their osseous system, of their extensive respiration, their high temperature, their rapid circulation, and the increased energy of all their functions in early consolidating and anchylosing so many parts of their skeleton, we can still perceive indications of an inferior grade in many of its parts. The mode of articulation of the vertebræ with each other, and their union by a single condyle with the occipital bone, the permanent separation of the os quadratum and the pterygoid bones, the chelonian form and covering of the jaws, and the great magnitude of the orbits, the great development of the coracoid bones, and the constant existence of false ribs anterior to the true, the imperfect development of the hand for prehension or solid support, and the permanent separation of the pubic bones, remove these animals from the type of the mammalia, and connect them with inferior tribes. So that while nature appears thus suddenly to bound forward to a more elevated and more perfect form of the skeleton in this class, we can still by a careful and extended comparison of its parts, trace the successive steps of the same regular and uniform plan of development which she has undeviatingly followed in the perfecting of individual systems of the economy, and in the construction of all organic forms.

LECTURES ON SURGERY,

BY

MR. WARDROP.

OF THE SANGUINEOUS SYSTEM.

ABSTRACTION OF BLOOD.

(Concluded from page 711.)

Causes of Failure in Venesection,—how to be remedied.—Operation of Venesection.—Injurious Effects of Blood-letting,—immediately after Injuries,—during a fit of Apoplexy,—in particular Cases of Inflammation,—in irregular Distributions of Blood—in Specific Diseases.—Different Methods of Abstracting Blood;—When to be employed,—by Venesection,—by Cupping,—by Leeches,—by Scarifications.

Causes of Failure in Venesection,—how to be Remedied.

In endeavouring to point out the indications by which the quantity of blood should be regulated when blood-letting is had recourse to in the treatment of diseases, it has been taken for granted that the surgeon has it always in his power to

abstract whatever quantity he may deem necessary. This, however, is not always the case, or at least the requisite quantity of blood is not always obtained without some difficulty, and nothing is more common than to hear of patients suffering from want of losing a sufficient quantity of blood, affirming that it was impossible to bleed them to the desired extent.

I have always considered it a dangerous maxim to admit the possibility of such an occurrence; and have ever contended that any quantity of blood deemed necessary can by some means or other be taken from every patient. Should the veins in the arm be inadequate for this purpose, then may the jugular veins, or the veins of the wrist or the feet, be opened, or, in cases of danger, when no time ought to be lost, why should not an arterial trunk be opened? The most severe operations are daily resorted to to save life, and surely a fellow-creature should not be allowed to die for want of the loss of a quantity of blood! In the *Transactions of the Med. and Chir. Society*, there is a case narrated wherein the necessary quantity of blood could not be obtained from the veins of the arm, and where the bold and decided practice of opening the radial artery was successfully adopted.

I recollect only one instance where I could not obtain by venesection the desirable quantity of blood, the patient becoming so extremely cold that scarcely any blood would flow from the wound, and this had happened to her on many former occasions.

It ought also to be remembered that when neither venesection nor arteriotomy can be performed, a large quantity of blood may be abstracted by leeches or cupping. The French are in the habit of applying at one time from fifty to a hundred leeches in cases where we would employ venesection.

The Operation of Venesection.—It will here be proper to allude to some circumstances, which, from a want of due care in performing the operation of venesection, are frequently the cause of failure in procuring the requisite quantity of blood.

The operation of blood-letting seldom has that degree of attention paid to all its details which it so well merits, and although every one considers himself perfectly capable of performing so simple an operation, still it cannot be denied that it is often executed by one surgeon much better than by another, and is, indeed, seldom perfectly well performed. The incision in the vein is either too small or unnecessarily large — the bandage too tight or too loose, or some circumstance essential to the success of the operation, however trifling it may appear, is not duly attended to. The assertion of the late Benjamin Bell, in his valuable system of surgery, does not go too far when he affirms, that, "Whilst I have frequently seen every other operation in surgery well performed, I have seldom seen blood-letting, with the lancet, correctly done."

I would strongly recommend to you the study of his writings on this subject, and to take care, when performing the operation, that you attend strictly to every step,—that you select the largest and most superficial vein in either arm,—that the bandage is applied neither so loose as not sufficiently to compress the veins, nor so tight as to diminish the flow of blood through the arteries,—and also that the inferior edge of the bandage be close to that part of the vein where the wound is to be made,—that the lancet be perfectly sharp, and the wound in the vein sufficiently but not too large,—that the patient be placed in an easy and recumbent posture, having the arm placed upon a common hand-basin, one edge of which will support the arm, and the other the fore-arm, and thus prevent the patient being fatigued, and also the orifices in the skin and vein will always be made to correspond.

All these circumstances demand attention in performing the common operation of phlebotomy, and as so much depends,—even the life of a patient,—on the prompt and successful manner in which it is executed, you cannot bestow on it too much consideration.

Injurious Effects of Bleeding.

Hitherto those classes of diseases only have been pointed out, wherein bleeding may be employed as the chief curative means; but there is another class of cases wherein this energetic remedy ought to be used with much caution.

Immediately after an Injury.—There is a class of cases in which blood-letting is often employed very unnecessarily, and often, too, perniciously. I allude to the common practice of bleeding persons immediately after an accident, or during an apoplectic or convulsive fit. In many accidents, more particularly where the head suffers, the first effects of the injury are a diminution or collapse of the vital powers; and if, under such circumstances, blood-letting be had recourse to, a still further diminution of the vital powers is produced. It is not until the powers of life have revived, or that a reaction has taken place, that you should, after severe injuries, employ blood-letting, and even then it

ought to be had recourse to with great circumspection.

Case.—The servant of a medical society getting a fall at the time of one of the meetings, and whilst still in a state of insensibility, was bled. It was at least fifteen months after that bleeding before she recovered her strength, although it was very moderate in quantity. Indeed, I suspect that many patients have suffered, after accidents, from a too early use of the lancet.

During an Apoplectic Fit.—In cases of apoplexy, I believe that blood-letting has frequently been carried to a very unwarrantable, and even fatal extent.

In proportion to the violence of an apoplectic shock, so are the powers of life diminished; and hence, if the quantity of blood abstracted be regulated by the severity of the symptoms, in like proportion will it be hurtful by still farther diminishing the vital powers. When a person is in a state of insensibility from an apoplectic fit, those around are too apt to urge the necessity of bleeding, conceiving that the loss of blood will cure the disease in the head, of which the fit is merely an effect or symptom.

Case.—A surgeon was sent for to see a patient, who had fallen down suddenly in an apoplectic fit; he immediately opened a vein, and after abstracting only a few ounces of blood, the pulse sank. When he visited this patient, several hours afterwards, he found the powers of life so feeble that he ordered him cordials, by which the action of the heart and arteries began to revive. Soon after this, however, another surgeon was called in, and he repeated the venesection, after which the patient sunk rapidly, and in a few hours expired.

There are cases of plethora or congestion in the brain, producing a sudden loss of the intellectual powers and convulsions, in which too much blood can scarcely be removed to save life; but in such cases the pulse is strong, usually acquiring vigour whilst the blood is flowing from the vein.

Case.—A general officer, of a full plethoric habit, and who had suffered occasionally from gout, had, in consequence of a slight pain in the great-toe, taken a brisk purgative, and whilst walking on the following day, which happened to be unusually cold, he felt a chill, suddenly became giddy, and fell down in the street, senseless and motionless. I found him in this state, but his pulse, though strong, was little changed. I immediately opened a vein in the arm, and the blood flowed freely through a large orifice; at first the pulse gradually acquired strength, and increased in frequency, but was not subdued until upwards of forty ounces of blood were abstracted, when he immediately became sensible to things around him. In this state he was removed to his own house, and in a very few hours the vigour of the heart and arteries revived, accompanied by uneasy feelings in the head, when about the same quantity of blood was taken away as on the former occasion. These depletions, along with a continuance of an antiphlogistic regimen, were the means of producing permanent relief.

In cases where there are organic changes in the brain's structure, and when the sudden apoplectic attack is caused by some vessel of the diseased part giving way and pouring out blood, blood-letting is of no avail, and when had recourse to when the pulse is feeble, and the vital powers already are much diminished, it never fails to hasten the patient's death: therefore in such cases blood-letting ought to be resorted to with great caution.

Case.—A lady considerably advanced in life was found during the night lying on the floor,—her servant, who was in an adjoining apartment, having been awoke by the noise of her mistress falling out of bed. She was perfectly insensible, and the pulse so languid, that the surgeon who was sent for ventured to take only a few ounces of blood by cupping; her pulse never revived, and in four hours she expired. It was found that her death was occasioned by an effusion of blood into the ventricles, the heart and aorta being also diseased.

It is in cases of this description that blood-letting, even to a small extent, must, by diminishing the powers of life, prove injurious, and, if carried further, inevitably hasten dissolution.

In particular Inflammatory Cases.—There are also individuals in whom the loss even of a very small quantity of blood produces great exhaustion and depression, and where it is impossible to carry depletion to the ordinary extent, even when they are labouring under inflammatory diseases. But you cannot possibly be aware of such constitutional peculiarities, unless by previous observation on the patient.

Case.—A lady about the middle period of life, who had suffered much from bad health, was suddenly attacked by all the usual symptoms of peritoneal inflammation. I found her in great agony, with a fixed pain in the abdomen, great tenderness to the touch, a very rapid pulse, and other symptoms of fever. Having taken repeated doses of opium without relief, I opened a vein in the arm, and before five

ounces of blood were abstracted. the pulse sunk and she fainted. When I visited her two hours afterwards, she had recovered from the state of syncope, and her pulse was revived, but very rapid and easily compressed. The local pain not having abated, the bandage was replaced on the arm, and scarcely had a couple of ounces come away when the pulse sank, and she again became faint. The pain in the abdomen not being relieved, after having recovered from the state of syncope produced by the second bleeding, I gave her a grain of opium with five of calomel every few hours, by which means the pain and other symptoms were rapidly subdued.

Another nice point to determine in the employment of blood-letting, occurs in those cases of inflammation where the powers of life are already so much exhausted from the duration of the disease, that though there may be a probability of the inflammation being subdued by bleeding, yet the exhaustion produced by any further depletion may of itself destroy life. Under such circumstances, therefore, in place of blood-letting, recourse should be had to those other means which art possesses to control the action of the heart and arteries. I allude to the use of large and frequent doses of the tartrate of antimony, to mercury combined with opiates, and to colchicum.

It often happens, that in cases where bleeding has been employed, the local pains accompanying the disease do not subside, and symptoms continue, or succeed, just as severe as those which preceded the venesection.

In such cases it is also a nice point to determine how far bleeding is to be carried; and some men of experience and accurate observation are very acute in perceiving those symptoms which can be relieved by blood-letting, and those which can be removed by blisters, anodynes, mercury and opium, colchicum, and such-like remedies.

In Irregular Distributions of Blood.—Bleeding will also be found more or less injurious when had recourse to in cases where there is merely an irregular distribution of the blood, or where there is an undue quantity in a particular part, without any increase in the quantity of the whole mass. Such cases are quite different in their pathological characters, both from those of congestion and of inflammation, and these three different conditions of an organ ought to be accurately distinguished.

In *congestion*, the quantity of the sanguineous fluid is increased, or the vessels are in a state of plethora. In an *inflamed part*, a change of structure is going on, so that if you compare an organ in a state of congestion with one which is inflamed, and macerate each in water, the blood of the first is washed away, leaving the natural structure unchanged, whilst in the inflamed organ, besides an increased quantity of blood, a change has been going on in the structure of the organ, more particularly an effusion of a sero-albuminous or puriform fluid, into the cellular membrane. Where there is an *irregular distribution of the blood*, and an undue quantity sent to a particular part, there is a corresponding diminution of the blood in some other parts, and it is in such cases where the abstraction of blood is always useless and sometimes injurious.

We have examples of these irregular distributions of blood in affections of the head and chest. Persons suffering from bilious and aguish headachs, as they are usually called, frequently have a flushed countenance, and an increased action of some of the branches of the external carotid artery; but they are not accompanied by any of those changes in the pulse which indicate the use of blood-letting, and are marked by symptoms showing a diminution of blood in some other parts of the body, and more particularly in the limbs, by a painful sense of coldness in the feet.

Persons who have often had bilious headachs, accompanied with a flushed face, and for which bleeding has been ineffectually tried, may, however, also have feelings of uneasiness in the head of a different character, and of that description wherein bleeding is highly beneficial. I have known several serious practical errors committed from the surgeon not being aware of this circumstance, and treating a headach from congestion of blood like an ordinary sympathetic headach. And patients themselves, subject to headachs from derangement in their digestive organs, are apt to attribute every uneasiness in the head which they may at any time experience, to the same cause.

Case.—The late Dr. Baillie frequently suffered from headachs connected with chylopoietic derangement.; and on several occasions they were so severe, that he had been induced to try the effect of cupping, from which, however, he did not experience the smallest benefit. Perceiving one day some spectra or images floating before his eyes, accompanied with uneasy feelings in the head, he asked my opinion; and considering that these symptoms were of a plethoric character, and indicated congestion within the head, I recommended the abstraction of some blood. To this he at first objected, from the in-

utility of former bleedings; but on representing to him the difference in the character of the present symptoms, leeches were applied behind the ear, which gave such relief as to induce him to repeat their application on the following day, by which treatment the spectra disappeared, and he was perfectly relieved from all uneasy feelings in the head.

Case.—A gentleman, who had often suffered from what he called "blind headachs" during forty years of his life, consulted the surgeon who lived in his vicinity, relative to some feelings in the head. Considering these symptoms to be unconnected with the "blind headachs," he advised him to be bled at the arm, which advice, however, was not followed. He now consulted a physician, who recommended to him the use of the sulphate of iron; and another physician, whom he afterwards consulted, recommended tonics, wine, and a generous diet. Having pursued this plan, and while he was on a visit to London some weeks afterwards, I was hastily sent for to see him, when I found that one side had become paralytic, with every symptom of congestion in the brain, and for the treatment of which he required repeated blood-letting.

In Specific Diseases.—In diseases usually called specific, such as syphilis, cancer, scrofula, gout, and rheumatism, blood-letting is often injurious, though it is sometimes useful when employed within certain limits.

Whatever organ is affected with acute rheumatism, there are always accompanying symptoms of an inflammatory type. Yet it is well established, that persons suffering from this disease cannot bear copious bleedings, and its too frequent repetition has certainly, in many instances, laid the foundation of chronic affections, more particularly of hypertrophy, or thickening of the muscular structure of the heart.

The same observation applies to persons affected with gouty inflammation in any organ. You will find that, in some cases, a certain extent of depletion will mitigate the inflammatory symptoms attending gout, but if the depletion be carried beyond a certain extent, it no longer alleviates the pain; the whole system seems to suffer, and the local disease is apt, as in rheumatism, to assume a chronic and unyielding form.

With regard to blood-letting in specific diseases generally, I may here observe that there is often not only the specific inflammation to contend with, but there is also more or less common or idiopathic inflammation accompanying the specific disease, and it is this idiopathic inflammation which ought to be treated by blood-letting, the bleeding having no influence in subduing the specific disease. It is not uncommon to observe a specific inflammation, once excited, thus cause an accompanying idiopathic inflammation.

In syphilis, for example, besides the primary ulcer, a great degree of inflammation of the adjacent parts sometimes supervenes, which inflammation is relieved by bleeding, and would be aggravated by the use of mercury. After this idiopathic inflammation is removed, then you can proceed to the cure of the specific disease by mercury. So it is in cancerous affections; the tension and swelling accompanying a scirrhous tumour can often be relieved by general and local blood-lettings. In proof of such bleedings being proper under certain circumstances, may be noticed the spontaneous hemorrhages to which such diseased parts are subject, and the effects of blood-letting in relieving certain states of plethora and congestion of the adjacent viscera.

The same remarks apply to the treatment of Gout. When gout affects any part of the body, it is often accompanied by an inflammatory swelling and pyrexia, which are relieved by bleeding, but the bleeding has no influence on the specific affection, and hence the usual practice in gout, of first subduing any severe local inflammation and pyrexia by depletion, and then and not till then, administering the specific remedies for the cure of gout.

The extent to which blood-letting, whether general or local, is to be carried in different specific diseases, and the particular circumstances which render these necessary, will be pointed out when that class of diseases comes under particular consideration.

Different Operations for abstracting Blood; when to be employed.

Before directing your attention to the comparative effects and the comparative advantages of abstracting blood by each of the several different operations of venesection, arteriotomy, cupping, leeches, and scarifications, it is of importance to keep in mind those states of the vascular system which require the use of this remedy.

I have already mentioned that whenever local inflammation exists so severe as to excite fever, venesection is then decidedly preferable to any other mode of abstracting blood. I have also endeavoured to point out what appeared to me to be the disadvantages of arteriotomy, and the few cases and particular circum-

stances to which that mode of depletion is applicable.

By Cupping.—In all cases of local inflammation where the general system is not disturbed, leeches are preferable to venesection; whilst the operation of cupping holds as it were a middle place, being applicable in cases where there are no febrile symptoms, and where leeches might be employed, as in local inflammations and congestions, and also in cases where the action of the vascular system is increased. In all inflammations and congestions about the head, cupping on the back of the neck and between the shoulders is a most useful mode of abstracting blood, and this operation is also particularly applicable for the removal of blood from the parietes of the chest and abdomen in diseases of the different viscera. Its use, however, is only admissible when it can be employed without exciting pain, and irritating the diseased organ. In such cases I have often seen cupping do mischief. For the same reasons cupping inflamed joints, or parts accompanied by external tenderness, is often injurious, so that in such cases leeches are decidedly preferable. Cupping is also preferable in those cases where the application of leeches is followed by severe erysipelas of the skin. The very circumstance of the cupping-glasses irritating the tender parts, and drawing blood from the surrounding parts to the place where they are applied, renders it an eligible mode of abstracting blood in cases of congestion, more particularly of the brain, the glasses being, in such cases, applied to the nape of the neck, whilst cupping is the least preferable mode of abstracting blood when the application of the glasses excites pain, and the influx of blood to the diseased part is increased. Although, therefore, the operation of cupping may be, under many circumstances, a useful and even most preferable mode of abstracting blood, yet it cannot be denied that there are no cases where cupping can be useful in which all the essential benefits of abstracting blood by that operation might not be obtained either by venesection, or by leeches, or by a combination of both of those methods. I have often remarked, that in this metropolis, where there are so many dexterous performers of this operation, persons, more particularly those subject to plethora in the head, are apt to indulge in a luxurious system of living, the injurious effects of which they can at all times have readily removed by cupping.

By Leeches.—Whilst there are diseases which can only be properly treated by venesection, there is also another description of cases, wherein the application of leeches is preferable after general bleeding or cupping.

Leeches are by far the most eligible mode of local blood-letting in the generality of cases, not only because they procure the necessary quantity of blood from the parts where the application of the cupping-glasses would cause great pain and irritation, but also because, from the great dexterity required to perform the operation of cupping, it is seldom well done, and cannot be commanded in situations and at times when leeches can easily be procured.

Leeches are in a more especial manner preferable in all cases of local injury, before the accession of fever. When a part is bruised, the whole frame receives a shock in proportion to the severity of the accident, and no sooner does the system recover, and reaction take place, than the bruised part becomes more or less tense and painful, which symptoms indicate the commencement of inflammation. If in this condition a sufficient number of leeches be applied to the parts, the inflammation will be immediately subdued, and if they be again and again applied, without delay, and in numbers proportionable to the severity of the symptoms whenever there is any return of pain and swelling, you can thus completely check and subdue every inflammatory symptom, and avert all the evils of the accident.

Case.—A gentleman fell from his horse, and severely bruised the elbow-joint and surrounding soft parts. Leeches were employed immediately after he recovered from the shock occasioned by the injury, which, along with fomentations and poultices, afforded great relief; but whenever the bleeding ceased, pain and a feeling of tension returned. More leeches were then applied, and so on, one or two dozen being repeated whenever the bleeding from the former wounds ceased and the pain returned, so that in four days one hundred and eighteen leeches were applied. The effect of this practice was, that in a week this patient had completely regained the use of his arm, and, except the debility, from which he recovered in a few weeks, no bad effects ensued. In this manner, and on this principle, have I, by the repeated application of leeches, kept up a constant oozing of blood from injured parts, until every inflammatory symptom has been completely checked and subdued.

When this system of treatment is not employed early,—almost immediately after the effects of the shock have passed away, and time is given to the inflammatory symptoms and a febrile disturbance of the system to supervene, then the treatment

must commence with general bleeding, conducted on the principles which I have already endeavoured to establish.

When, besides a bruise, the soft parts are wounded, the subsequent hemorrhage often goes far in checking the approach of inflammation; and hence, when wounds have bled freely, and when they have not been irritated by dressings and bandages, it is seldom that any inflammatory symptoms ensue, nor does any local or general bleeding become necessary. When speaking of the advantages of bleeding during operations, I also endeavoured very fully to explain the importance of such hemorrhage in checking inflammation in wounds.

When leeches cannot be procured, the requisite quantity of blood may be obtained by opening with a lancet one or more of the veins contiguous to the injured parts; and this mode of blood-letting is particularly applicable in injuries of the extremities, where there are so many accessible and sufficiently large veins. The advantages of applying leeches, which have been enumerated, are also to be derived from this method, in all local affections where the parts from which the blood is to be withdrawn, are tender to the touch, and where any other mode of local blood-letting is inadmissible.

In the application of leeches, it is important to be aware that the bleeding from their wounds is never in the ratio of the number applied; and you will often be surprised to find three or four leeches bleed as much on one occasion, as at least double the number on another. Usually, when a large number is applied, the blood flows most freely from a limited number of wounds, which continue to bleed long after the bites of the others have ceased. This probably depends on the principal vessels supplying the part on which the leeches are applied, being wounded, and those wounds which happen to be made nearest the heart, bleeding more than those in the more remote branches. The bleeding too from leeches varies much in quantity, not only according to the state of the circulating system generally, but also according to the degree of inflammation or congestion in the part where they are applied; and very remarkable is the difference in the bleeding from the same number of leeches on the same part at different times, the quantity diminishing as the fulness of the vessels is diminished. This fact coincides with the observations I made on the quantity of blood necessary to produce syncope in inflammatory diseases, which I remarked was always in proportion to the quantity necessary for removing the disease.

Case.—I was consulted by a surgeon on the case of an old lady who had all the symptoms of hypertrophy of the heart, the impulse of which organ had become extremely vigorous, with excessive embarrassment in breathing. In place of venesection, I recommended the application of four leeches on the region of the heart. The leech-bites continued to bleed freely during fifteen hours, subduing both the unnatural vigour of the pulse and the action of the heart. Two days afterwards the same number of leeches were again applied, and, to the astonishment both of the patient and her other medical attendant, but a very small quantity of blood came away.

Leeches, too, bleed very differently at different seasons and in different states of the atmosphere, over which we have no control. It is also of importance to keep in mind, when comparing the effects of cupping and leeches, that advantages are derived from the long-continued fomentation of the affected parts to which the leeches have been applied, as also to the soothing influence of a poultice, which ought always to be used after the fomentations. One disadvantage of the use of leeches is the difficulty of always regulating the extent of the bleeding, but I have already observed, that the discharge from their bites usually depends on the degree of congestion of the vessels of the part to which they are applied, and that they seldom bleed more than is requisite.

When it becomes from any cause desirable to put a stop to the bleeding, this can in most cases be readily effected by simply exposing the surface of the part to the external air, and allowing the blood in the orifices to coagulate. The usual practice of applying compresses and bandages, and heaping them on in proportion to the activity of the bleeding, tends to promote instead of checking the flow of blood, as shall be more particularly explained when treating of wounded blood-vessels.

Case.—I was sent for early one morning to visit a lady who had violently sprained her wrist on the preceding day, and for which she had applied leeches. They had bled freely all the evening, and so profusely during the night, that a variety of styptics, compresses, and bandages, were used, but without arresting the hemorrhage. The injury having been severe, and the pulse not being altogether subdued, I told her that she had not lost too much blood, and that the constant oozing from the leech-bites had been the means of preventing the accession of inflammation. The patient being alarmed, how-

ever, I thought it expedient to arrest the bleeding, and scarcely had the bandages and thick compresses been removed, all of which were soaked in blood, and the hand and arm exposed to the cool air, than the flow of blood began to diminish, and in a few minutes was completely stopped.

When this simple means is not sufficient, you will seldom fail to stop the bleeding from a leech-bite, by applying a small compress not larger than a finger-nail, and consisting of several folds of lint, directly upon the bleeding orifice, on which such a degree of pressure is to be made with the point of only one finger as shall be found sufficient to stop the bleeding, and this pressure is to be steadily continued until all tendency to bleeding has completely ceased. I have never known this practice to fail, except in a few cases where the hemorrhage arose from the blood wanting its usual coagulating power, and in some children where from the leeches having been applied to the neck, and more particularly where the leeches were of a large size, the necessary degree of pressure could not be employed. In the first class of cases, touching the orifice with the nitrate of quicksilver produces an immediate coagulation of the blood, with which it mixes, and thus plugs up the wound. Caustic may be in like manner employed in hemorrhages from leech-bites in children; and Sir Charles Bell has, in troublesome cases of this kind, stitched the lips of the wound together with a fine needle and thread, or the needle may be allowed to remain for some hours in the wound, and its thread twisted round it, as is done in making the twisted suture, and thus a firm and unyielding compress is formed in the wound.

By Scarifications.—There is no class of diseases where the effects of the local abstraction of blood are so well exemplified as in some of those of the eye, in which a quantity of blood may be taken from the conjunctiva by the operation of "scarification."

The beneficial effects of applying leeches to the orifices of the different mucous canals, when depletion is required, is well established, and the operation of scarifying the conjunctiva, lancing the gums, puncturing hemorrhoidal tumours, and making incisions in inflamed parts, all give relief on the same principle.

Scarifying the conjunctiva is a most useful operation, and one of the few methods we possess of abstracting blood locally, strictly so speaking. Simple as may be its performance, there are still several circumstances requiring nicety in this operation, and which are necessary for its success; and in the common mode in which it is done, instead of doing good, it often irritates and produces mischief. This operation was known to the Arabians, who performed it by inserting the serrated edge of the beard of the common barley into the conjunctiva, the wounds thus made bleeding more or less freely. Long ago I used an instrument made on the same principle, consisting of a thin blade of steel, with a serrated edge well sharpened, and each tooth forming an acute angle with the blade. But the most safe and efficient mode of performing this operation is with a wedge-shaped scarificator now in common use. The lancet is a very unfit instrument for this purpose.

The principle of the operation is, by wounding the conjunctiva, to get as much blood as possible. The wounds should only be made on the palbebral, and never on the sclerotic, conjunctiva, as when the vessels on the ball of the eye are divided, the operation invariably excites much more irritation than the loss of blood does good. The wounds, therefore, should be limited to the conjunctiva lining the eyelids, and it is only on the inferior lid where the scarification can be properly accomplished, and the wounds or scratches should be made on that part of the membrane which is reflected over the cartilaginous tarsus. All its other portions are loose and unresisting, whereas on the tarsus the conjunctiva is kept stretched, and the cartilage affords a resistance sufficiently firm to admit of the ready division of the blood-vessels.

Before using the scarificator, you should completely evert the lower eyelid by the fore and middle finger of the left-hand, underneath the points of which is placed a thick dossil of lint, and this assists in keeping the lid everted,—absorbing the blood,—and also by compressing the lid on the lower edge of the orbit, the blood is made to flow much more freely.

The edge of the scarificator is to be held perpendicular to the surface of the conjunctiva, and the weight of the instrument is of itself sufficient to make the wound deep enough. One scratch, for it ought not to be an incision, along the tarsus, generally answers the purpose, but several are sometimes necessary. You can, however, easily comprehend how one division of the same vessel should bleed as much as several. By retaining the compress on the inverted lid, pressed moderately, it is often surprising the quantity of blood which is discharged, and the relief and benefit of the operation are generally in proportion to that quantity, just as has been observed when speaking of the blood taken by

leeches being in proportion to the turgescence of the vessels where they are applied.

It is worthy of remark, that the abstraction of blood by scarifying is never useful until the acute stage of the inflammation has passed. It is in the subacute, or chronic, or passive stage wherein such local depletions appear so decidedly useful, in this respect resembling the relief in congestions from spontaneous hemorrhages. When the eye is suffering from an acute inflammation, the vessels of the palpebral conjunctiva are not much increased in number, and appear chiefly arterial, and when divided yield scarcely any blood. But when the second stage of the inflammation ensues, there is a great increase in the number and change in the colour of the vessels, the arterial capillaries being diminished in number, and the venous increased, while the bright red colour becomes of a more purple hue.

STRICTURE OF THE RECTUM.

REJOINDER OF MR. SALMON TO DR. O'BEIRNE.

To the Editor of THE LANCET.

SIR,—My observations on Dr. O'Beirne's last letter shall be as brief as his accusations and numerous misstatements will permit. He commences, "*Mr. Salmon's reply is a general one, embracing a variety of points, calculated to divert the mind from the subject of organic stricture, upon which we are mainly at issue, and which we had mutually agreed to leave to the decision of two distinguished members of the profession*;" thus insinuating that I had introduced new matter for controversy. Now, a reference to my letter of November the 30th will prove that I confined my answer to such points only as Dr. O'Beirne had advanced; my reply, therefore, was not general in the sense he implies, though it was a general, and I think I may add, from the discomfiture he manifests, a satisfactory one, to all his positions.

I will endeavour to make my present letter alike conclusive to all unprejudiced persons. *Imprimis:* I must express my regret that he has again compelled me to exculpate myself from charges conveyed in so sophistical a manner that they carry with them the appearance of truth, though (as I shall presently show) they possess not the smallest portion of that inestimable principle. I am accused of a "*positive breach of contract.*" A simple narrative of facts will convey a full and unequivocal refutation of this accusation. In my letter published in the *London Medical and Surgical Journal* of the 20th of April, I undertook to prove, by preparations in my possession, the existence of permanent stricture in the lower part of the rectum, "and offered to show them to any members of our profession." Dr. O'Beirne in substance answered, that if either of his friends, Mr. Guthrie or Mr. Bransby Cooper, would confirm what I had said, he would believe it, retract his opinions, and admit, what he has even in his last letter again denied, the existence of that disease. Considering the point was of importance to science, I complied with Dr. O'Beirne's challenge, and (as I before stated) immediately wrote to Mr. Cooper; I however thought proper to ask Dr. Blundell to meet him. Both of these gentlemen politely acceded to my request, and an appointment was made to inspect the preparations. Dr. Blundell and Mr. Cooper did not, however, meet; at least not with my knowledge, the former gentleman being behind his appointment. I had selected for their inspection fifteen preparations of stricture of the rectum only. Mr. Cooper took a cursory view of them and left, saying, he supposed he should see or hear from Dr. Blundell. Upon this gentleman's arrival, he also inspected the preparations, and desired certain ones which he selected to be sent to his residence. I heard nothing more, directly or indirectly, of Mr. Cooper till I wrote to him for his opinion a day or two before the date of the first of his letters herewith published; neither did I hear from Dr. Blundell till the day before I wrote my reply to Mr. Oldknow, which appeared in your journal of November the 16th, when I read a rough copy of what Dr. Blundell had formed as his opinion. His final judgment I only received, as our letters show, on the 30th of November. From these facts it is clear that Dr. O'Beirne and I never "*mutually agreed to leave the matter to the decision*" of Dr. Blundell and Mr. Cooper, for the former gentleman was not mentioned, nor would he have ever been heard of in the affair till the publication of my last letter, had not Dr. O'Beirne been apprized of his having inspected the specimens by Mr. Cooper. I further "*submit to the profession*" that had I chosen to obtain the opinion of fifty individuals upon the character of the preparations, I was at perfect liberty so to do, neither could such a course injure Dr. O'Beirne, as those persons could only testify from ocular demonstration of a matter of fact, the confirmation or refutation of which ought to be his first consideration. He

complains of the time which elapsed before the decision was made public. Now, for such delay I apologized, stating, "The truth is, I have not had the documents necessary to complete the answer till very lately (which fact a reference to the accompanying correspondence confirms), "added to which my leisure has been completely occupied by the affairs relative to my resignation at the General Dispensary;" after that explanation it was anything but kind in Dr. O'Beirne to state that there was a "hitch somewhere." He proceeds:—"*I had the favourable documents*" (Dr. Blundell's and Mr. Cooper's decisions) "*so early as the middle of November;*" but he was too scrupulous, it appears, to expose my ignorance by giving them publicity. Favourable, however, as he would lead us to suppose he thought them, we nevertheless find him writing to Mr. Cooper, and suggesting the "*necessity of slitting the preparations marked A and B their whole length before either he or Dr. Blundell could come to final decision.*" "*But it* WOULD" (not it *does*) "*appear that Mr. Cooper was prevented from making this fair and considerate proposal, by a step which Mr. Salmon had taken in the mean time, namely, requesting Mr. Stanley and Dr. Ryan to inspect his preparations.*" The extreme modesty of Dr. O'Beirne's proposition every curator of anatomical preparations will fully appreciate, and it was no wonder that a gentleman of the correct feeling Mr. Bransby Cooper is known to possess, abstained from making it; unfortunately, however, for the Doctor, it so happens that the preparations have been slit open, so as to exhibit their diseased condition, ever since they were put up; nevertheless, the reason assigned by him why they were not so mutilated is contradicted by Mr. Cooper's correspondence, which clearly proves, that the final decision of Dr. Blundell and the former gentleman were in Dr. O'Beirne's possession many, many days before his destructive proposition was requested to be made. From the manner in which he speaks of the reason I assigned for requesting Mr. Stanley and Dr. Ryan's judgment, your readers would gather that I meant the words "finding they disagreed so materially," to apply to some matter of conversation between Dr. Blundell and Mr. Cooper, though Dr. O'Beirne must have known that the words were used in reference to their recorded opinions, as my letters to the latter of these two gentlemen likewise illustrate; and as to Dr. O'Beirne's assertion that those documents "*perfectly agree,*" "the merest tyro in the profession (to repeat his classical and elegant language) *has only to compare both, in order to be convinced that they are as widely different as light from darkness.*" I should forget the respect due to myself and your readers, even more than my opponent has done, were I to notice the paragraph wherein he charges me with not "*being over-squeamish or punctilious in availing myself of advantages,*" which exist only in his imagination.

He next proceeds to illustrate "*the exact nature and practical bearing*" of our controversy, which of course no one "*except his friend Mr. Cooper*" has fully understood; and to explain his own peculiar views, so as to "*succeed in placing the whole subject in a clearer, fuller, and much more useful point of view than the plan of*" his "*work admitted.*" Here we have a clear admission that his book, published for the avowed purpose of proving and promulgating his "*new views,*" is so unsatisfactory even to its author, that he feels it necessary to illustrate his ideas on one of its most important points by an explanatory letter in a weekly periodical. In furtherance of this illustration he gives "*the received ideas of the morbid conditions constituting organic and permanent stricture;*" not, however, by quoting my opinions on the subject, but those "*of the latest writer.*" Here I forbear making any further remark, except, that I account it both uncourteous and unfair to condemn or detract from the opinions of any individual, either publicly or in private, with whom I am not controversially engaged. Had Dr. O'Beirne extracted the morbid anatomy of the rectum from my work, the originality of his notions would have vanished into thin air; for he must have shown that I speak of circumscribed, partial, and general thickening of the rectum, and yet remark upon the healthy condition of its mucous tunic, in contradistinction to all authors who had preceded me. Dr. O'Beirne next misapplies my words, making it appear that I disavowed the existence of permanent stricture anywhere *but* in the lower part of the bowel. Now, he must have been aware that the words he has quoted were used in reply to *his* denying the existence of permanent contraction below the union of the colon and rectum; "a latitude of opinion," which, to use my former language even, "I had not given Dr. O'Beirne credit for, since I merely contended (that is, in my controversy with him) for permanent stricture in the lower part of the rectum."

He proceeds to say that Mr. Salmon admits "*that distinct, thickened, and shelf-like projections are exceedingly unusual in the two upper thirds of the rectum.*" Now I did not admit any such thing, but I did

infer it, and I now maintain that such is the fact, because, as I before said, "a uniform thickening," but not shelf-like projections, is the common morbid appearance of stricture in the two upper thirds of the bowels. Dr. O'Beirne's reasoning against circular stricture resulting from a redundancy of the circular fibres, is singularly scientific, since from it we are to infer that, because the action of the circular and longitudinal fibres is "*simultaneous relaxation and contraction*," the preponderating power of the former set of fibres is of no avail, and this too is asserted in the face of his own friend's decision, who says, "the specimen of distinct threadlike constriction which I saw producing the distinct circular contraction was spasmodic." Now, is it not clear, if such an effect can be produced from spasm, this, or the causes which gave rise to it, continuing, that the contraction would become permanent? Dr. O'Beirne asks, "*Are the statements just referred to sustained by the evidence of pathological anatomy?*" and in proof that such is not the case, writes, "*I shall now state both the morbid changes I have not observed, and those which I have observed.*" "*Except* AT THE UPPER EXTREMITY, *I observed no partial thickening of the parietes of the intestines;*" yet a little further on we find him admitting as appearances which he has observed "*all that great portion of the rectum above its pouch thickened in its parietes, and contracted in its cavity.*" Thus have his researches into the morbid anatomy of the rectum led him not only to contradict himself, but also fully to confirm my published opinions on that subject.

We are next favoured with a lengthened dissertation upon his "*new views*," the "*practical bearing*" of which he proceeds to show, also to prove the "*unscientific and dangerous nature of a practice evidently suggested by the erroneous notions of stricture;*" to wit, the division of the disease when situated near to the anus, an operation which he is kind enough to say is "*recommended*" among others by "*Mr. Salmon.*" A short extract from the chapter in my work which treats of the division of the stricture, will show how carefully he has perused, and how clearly he has comprehended my observations on this subject. "Taking into consideration the danger consequent upon hemorrhage, the probability of inflammation, and the distressing irritation which arises from the necessity of keeping the divided portions of the gut separated during the healing process, the division of a stricture may be always accounted a dangerous, and, in many cases, an equally useless operation. There are few instances where it can be attempted consistently with utility and safety." I then assign my reasons why I would not perform the operation, and direct "the reader's attention to the distinction between that kind of obstruction which is produced from the formation of adventitious bands or septa in the rectum, and stricture of the gut itself." The former of which, I state, "may be freely divided without any apprehension of danger." Having described the manner of performing the operation, I terminate my remarks by stating, that "upon the whole, the operation is not of a description to induce me to recommend its performance; such, at least, is the result of my experience. Under the most favourable issue I do not think it likely to be productive of that degree of success, which may be a considered a compensation for so hazardous and painful an expedient."*

Few of your readers would fail to suppose, by the plausible manner in which Dr. O'Beirne adverts to the subject, that because I deny "*shelf-like projections*," as a com mon appearance in the "*two upper-thirds of the rectum*," I deny the existence of stricture at those parts. Quite the contrary, as the sense of my writings unperverted demonstrates, wherein I say that "the most common situation of contraction in such cases as have fallen under my observation is between five and six inches from the anus." Next in frequency I have discovered the disease at the junction of the rectum with the colon; but the appearance of these obstructions will not be shelf-like projections (save at the angles of the bowel), but one uniform thickening of its parietes.

Although I am satisfied no one who is acquainted with the morbid anatomy of the rectum is likely to be led away by Dr. O'Beirne's argumentative attempts to prove the concordance of Mr. Cooper's judgment with that of Drs. Blundell and Ryan, and Mr. Stanley, I must not omit showing the clever manner in which he "*most gratuitously*" attempts to maintain this position by ascribing to me opinions I have never entertained. He asks, "*Are such appearances on the external surface of the bowel, the natural consequence of organic stricture?*" (alluding to the remarks of Drs. Blundell and Ryan, and Mr. Stanley). My reply is, yes; and further, they are proofs of the fact. The Doctor, however, says, "*No; even my opponent admits that this form of the disease is produced by the deposition of new matter between the mucous and muscular coats, and that this new matter, instead of approximating, separates these coats from each other,*

* Salmon on the Rectum. Fourth edition, p. 66.

and, consequently, opposes the formation of anything like an indentation on the external surface of the gut." Now what do I really say upon circular stricture of the rectum within reach of the finger, the point upon which we are here at issue? "We sometimes find bands of a firm consistence extending from side to side, forming septa across the bowel; these septa are most commonly situate towards the inferior part of the gut. Then again, we find the intestine contracted in a circular form, being dilated immediately above the contracted part by the perpetual lodgment of feces, in the form of an hour-glass. This is by far the most formidable kind of stricture, simple contraction, as well as the most difficult of alleviation. But the peculiar and morbid changes generally formed in cases of confirmed stricture, appear to be the result of considerable thickening of the bowel, from deposition in the cellular tissue which connects the muscular and mucous coats." *

I will not comment any further upon these "*similar*" opinions than to add, that Dr. O'Beirne has a happy knack of noticing what may by possibility be distorted in his favour, and of carefully avoiding what satisfactorily refutes his positions. Thus we find, that of Mr. Cooper's decision of the spasmodic annular stricture (fully proved by Drs. Blundell and Ryan, and Mr. Stanley, to be a case of permanent obstruction), he says, "*Mr. Cooper in a letter to me states, that the contraction is evidently not the change of structure.*" What, I would ask, have your readers to do with Mr. Cooper's private correspondence with Dr. O'Beirne? It is the former gentleman's recorded opinion we are to be guided by. Then, again, he says, "*Mr. Stanley, under the head of second specimen from a male, is less accurate in his account of this preparation;*" thus taking for granted that it was the same Dr. Blundell described under preparation *B*, which it was not, but a second case of circular stricture within reach of the finger. Had Dr. O'Beirne read the description with the slightest attention, he must have seen this. He likewise carefully avoids alluding to the conclusion of Dr. Ryan's judgment, "From these and the other preparations, I am perfectly convinced of the existence of stricture in every part of the rectum." So also of Mr. Stanley's judgment of the third case, he assumes it to have been one of thickening from fistula; unfortunately, however, I can attest to the contrary, having attended the patient from whom the preparation was taken nearly two years before any fistulæ occurred with circular stricture, which disease I have reason to suppose was produced from the extreme cold to which he was subjected, while with the expedition to the North Pole, of which expedition he was one of the officers.

I have thus, I believe, exposed the fallacy of Dr. O'Beirne's arguments, shown the untenability of his positions, as well as I trust satisfactorily refuted his accusations. The performance of this disagreeable task has caused me some pain, but the personalities to which my opponent descended, left me no alternative, and I have endeavoured "*to perform it in as respectful and moderate a manner as the circumstances will permit.*" And now, convinced that your readers, like myself, must be pretty well tired of this controversy, I beg leave to decline extending it under *any circumstances whatsoever*; gratefully thanking you for the space you have accorded me in the pages of your valuable journal. I remain, Sir,

Your very obedient servant,

FREDERICK SALMON.

12, Old Broad Street, Feb. 6, 1834.

Mr. Salmon to Dr. Blundell.

MY DEAR SIR,—I request you will be so good as to give the bearer (if it is ready), or if not, send me by Monday morning's first post, your opinion on the preparations; I am to send it to the printer's by ten o'clock on that day. Mr. Bransby Cooper has sent the one you wrote for him away to O'Beirne, so that I have no means of saving you this additional trouble. I am, my dear Sir, yours, &c.,

FREDERICK SALMON.

12, Old Broad Street, Nov. 30, 1833.

Dr. Blundell in reply.—My dear Salmon, excuse haste.—J. B.—(Opinion returned in my note. —F. S.)

No. 1.—*Mr. Cooper in Reply to a Note of Mr. Salmon, requesting his Opinion of the Preparations.*

My dear Sir,—I waited for Dr. Blundell's account of your preparations until I was tired, and as I did not like to keep Dr. O'Beirne so long in suspense, I wrote him my opinion of your preparations which I saw, and have since sent him the paper Dr. Blundell enclosed to me. I kept no copy of my own opinions respecting how far I considered your preparations held out, or, rather, demonstrated your views of stricture of the rectum, but I know that it went against yours, and in favour of Dr. O'Beirne's ideas upon the subject; but I will write to O'Beirne immediately, that I may forward to you my precise words as written under the first impression, or if the delay be inconvenient to you, I will write the substance again. Yours, very truly,

BRANSBY B. COOPER.

(No day or date, but received Nov. 20th, 1833.—F. S.)

No. 2.—*Mr. Salmon to Mr. Cooper.*

12, Old Broad Street, Nov. 29, 1833.

My dear Sir,—I regret that you should have sent your decision to Dr. O'Beirne without my knowledge, and confess I think that in a question so deeply

* Salmon on the Rectum. Fourth edition, p. 22.

involving the interests of the community, our profession, and my character, you ought not to have done so without first apprizing me of your intention. Neither does it appear to me that the reason you assign for having written to your friend (to wit Dr. Blundell's delay), alters the circumstances of the case, since, had you communicated with me, such delay would have been satisfactorily accounted for. This proceeding is, however, of minor importance, compared with the decision you state you have given upon the various preparations of stricture of the rectum submitted to your inspection, and which I must now, in justice to myself, lay before two or three other experienced morbid anatomists, notwithstanding the clear, decisive, and incontrovertible statement, Dr Blundell forwarded to you of them, and which fully illustrates the fact I publicly pledged myself to confirm. Meanwhile, oblige me by immediately writing to Dr. O'Beirne for a copy of your opinion, with the view of my giving it publicity, in conjunction with that of Dr. Blundell, and those of others whom I may hereafter request to inspect the preparations. Apologizing for giving you so much trouble, I am, my dear Sir, yours faithfully,

FREDERICK SALMON.

No. 3.—*Mr. B. Cooper to Mr. Salmon.*

My dear Sir,—As it was impossible that you and Dr. O'Beirne could simultaneously receive the opinion I had formed of your preparations, in reference to the point in dispute between you, I cannot see why he had not the same right to have it first as you seem to claim to yourself. Yours, very truly,

BRANSBY B. COOPER.

(No day or date, but received Nov. 22nd, 1833.—P. S.)

No. 4.—*Mr. Salmon to Mr. B. Cooper.*

12, Old Broad Street, Nov. 30, 1833

My dear Sir,—Being pressed for my reply to Dr. O'Beirne, I cannot any longer delay it, for the purpose of adding, as I could have wished, the opinion which you stated in your first note to me you had forwarded to that gentleman, and which I rather expected would have arrived from Dublin ere this. I must therefore, in justice to Dr. O'Beirne, publish our correspondence as illustrative of your decision. You appear to have mistaken the sense of my last letter; I only meant that I thought a copy of your opinion should have been forwarded to me as well as to Dr. O'Beirne; this however, it seems, could not be done, since you omitted to take a copy of it. I am, my dear Sir, yours, very faithfully,

FREDERICK SALMON.

No. 5.—*Mr. B. Cooper to Mr. Salmon.*

My dear Sir,—On enclosing you this opinion on the subject of your controversy with Dr. O'Beirne, I must leave it to yourselves to place the issue before the profession; and believe me, that in giving my opinion, I am actuated only by a desire to elicit truth. I have to apologize for not having sent this to you earlier, but I have wished to have met Dr. Blundell at your house, and have taken the preparations out of the bottles to examine them more closely. I do not, however, like to withhold my decision longer, although I should still wish to meet Dr. Blundell with you. Yours, very truly,

BRANSBY B. COOPER.

New Street, Sunday (Dec. 1st, added by P. S.)

No. 6.—*Mr. Salmon to Mr. B. Cooper.*

My dear Sir,—I have this moment, while at dinner, received your letter, enclosing an opinion respecting the preparations. I am unable to say any more (as your servant is waiting) than, that I have sent my answer to Dr. O'Beirne to the Medical Journals, also a letter to you last night, which fully explains why I did so. Am I to understand that the opinion I now receive is the copy of what you sent to Dublin to Dr. O'Beirne? Be so good as to reply to this question: also say whether you wish the communication I have just received, forwarded to the medical periodicals, in which my reply to your friend is to appear; I think if I send it before Tuesday morning, it may be added. Yours, very truly,

FREDERICK SALMON.

Sunday, December 1st, quarter-past 6 o'clock.

No. 7.—*Mr. B. Cooper to Mr. Salmon.*

My dear Sir,—With respect to my opinion of your preparations, which I have sent to you, you are at liberty to do with it as you please. It is what I wrote immediately upon seeing your collection, and you would have had them long since, had not Dr. Blundell been so tedious in his delay. I therefore sent it to O'Beirne, and now to you, as it was first written. Why you should write a letter to me through the public journals, when there is a two-penny post, is best known to yourself. I am yours truly,

BRANSBY B. COOPER.

New Street, Monday morning.

No. 8.—*Mr. Salmon to Mr. B. Cooper.*

12, Old Broad Street, Feb. 4, 1834.

My dear Sir,—I suppose you could not have received my note posted on Saturday night, when you forwarded yours to me of this morning; or, if you had, you have again sadly misunderstood the sense of it. I have addressed no letter to you through the press; all I have done is to forward our correspondence relative to your judgment of the preparations, as a matter of satisfaction to Dr. O'Beirne and the profession, an omission of which would have rendered my answer to that gentleman incomplete. I will send and stop (if possible) the publication of your correspondence, and also request that the opinion I yesterday received from you may be inserted in its stead. I am, my dear Sir, with good feeling, yours, very truly,

FREDERICK SALMON.

December 2nd, 1833.

No. 9.—*Mr. Salmon to Mr. B. Cooper.*

My dear Sir,—I am sorry to be under the necessity of publishing the correspondence which passed between us relative to my controversy with Dr. O'Beirne, but the accusations he has thought proper to make affecting my character, not only for consistency, but even common propriety towards you and himself, have, in my judgment, as well as in that of my friends, rendered this step absolutely necessary. I am the more surprised at the course Dr. O'Beirne has taken, since he implicates you by stating that he had the "favourable documents" containing the opinions of yourself and Dr. Blundell "so early as the middle of last November," but that instead of "taking any advantage of Mr. Salmon, he, Dr. O'Beirne, wrote to Mr. Cooper, and submitted to him the necessity of slitting the preparations marked A and B, their whole length, before either he or Dr. Blundell could fairly come to a final decision." Dr. O'Beirne then adds, "that it would appear that Mr. Cooper *was prevented from making this fair and considerate proposal, or interfering further in the affair, by the step Mr. Salmon had taken in the* MEAN TIME—namely, requesting Mr. Stanley and Dr. Ryan to inspect his preparations, and give their opinion on the contested points." Now the fact is, as you I feel assured will readily admit, that yours as well as Dr. Blundell's opinion was formed and forwarded to Dr. O'Beirne some days before I wrote to you, stating that I should request the judgment of some other individuals, which step I took, as our letters prove, because you were unable to forward me your decision on the preparations, having taken no copy of what you had sent to Dr. O'Beirne, but which you added was in his favour.

You will likewise remember that our correspondence was gone to the printer's, with my answer to your friend, when the copy of your judgment, which you had sent for to Dublin, reached me, and that I stopped the press to alter my manuscript, so as to make it

consistent with that judgment, and to substitute it for your letters.

Be so good as to give me a few lines in reply, stating whether the foregoing is not a correct representation of the facts of the case, which, if you would do at your earliest convenience, I shall be obliged, so that I may be enabled to give your reply publicity in the journals of Saturday next.

In conclusion I repeat my unfeigned regret for the course Dr. O'Beirne has compelled me to pursue, and I beg to assure you of the sincere respect and esteem of yours, very faithfully,

FREDERICK SALMON.

No. 10.—*Mr. B. Cooper to Mr. Salmon.*

My dear Sir,—If you think it necessary, you are perfectly welcome to publish the correspondence between us, relative to the controversy between yourself and Dr. Beirne. Yours, very truly,

BRANSBY B. COOPER.

2, New Street, Feb. 5th, 1834.

RICE AND CHOLERA.

REMARKS, BY DR. TYTLER, ON THE LETTER OF MR. DUNN.

To the Editor of THE LANCET.

SIR,—The letter of Mr. Dunn, published in the last number of THE LANCET, contains two facts of great importance. The first is contained in the following assertion of Mr. Dunn. "In November, in consequence of the circumstances mentioned in my former letter, an alteration in the dietary was made, by which the animal food was reduced; milk, which before was allowed to be purchased, was forbidden, and *two ounces* of rice were, *twice* a week, substituted for potatoes." This is followed by the remark, that "the quantity of rice allowed (two ounces twice a week) is too small *to have produced any effect.*" But, Sir, with submission to Mr. Dunn, these are neither the words of his former letter, nor do they convey the inferences which are deducible from that letter, as corroborated by the facts that are disclosed in the communication under date 4th instant. The words of his former letter are these: "We have substituted an allowance of rice, the result of which I will communicate after we have made a longer trial of it." Now, Sir, what is the result? And this information in his last letter is not *directly* afforded by Mr. Dunn, for he there states, "the quantity of rice is too small to have produced any effect," and yet in his preceding communication, we were promised to have the "RESULT" of the quantity of rice, *small or great*, which was given as diet to the prisoners at Wakefield!

Nevertheless this information *is* amply supplied, although *indirectly* supplied, in Mr. Dunn's recent epistle, because in it he unequivocally mentions that on the 9th of December, "a case of Asiatic cholera occurred in a vagrant," and that "the disease continued to spread among the vagrants until the morning of the 24th of January, when it made its appearance in a most alarming form, *without any premonitory symptoms*, in a different part of the prison." This, Sir, is a most accurate description of *rice disease*, as it often makes its appearance in those who are using rice, without any warning symptom, and this identical effect is thus alluded to in my letter of Nov. 4th, 1833. "The rice may therefore produce, should it be bad, a deleterious effect on those who partake of it, although the disease thus produced may appear to the medical gentlemen in attendance to originate in some other cause." In the Wakefield prison rice was given as food, and continued to be given, and in that prison, while the rice was so given, dreadful bowel complaints, *cholera*, and other diseases, made their appearance, and this is "THE RESULT" of "THE TRIAL" which has been "made with rice" at Wakefield, as is gathered from Mr. Dunn's own statements, although he says the rice was given only at the rate of two ounces per week.

The second fact alluded to in the letter of Mr. Dunn, which requires notice, is this. Mr. Dunn says, "In many of the samples furnished in Wakefield I detected the varieties of bad rice Dr. Tytler has described." Here, then, in Wakefield, an inland town of Yorkshire in England, the deleterious rice is *admitted to be present*, and that town has been conspicuous, at least since the discussions on the rice question, for the prevalence of *cholera*. Those facts speak volumes, and it is, therefore, unnecessary for me to pursue the subject further. I beg to remain, Sir, yours very obediently,

R. TYTLER, M.D.

London, February 16th, 1834.

APPLICATION OF

WHITE PAINT TO BURNS AND SCALDS.

To the Editor of THE LANCET.

SIR,—I have been induced by the success of the practice, to lay before you a brief account of a mode of treating burns and scalds which I have followed for some time with great and immediate relief to the patient. The application may not be new, but I have not seen it mentioned in any work that I have read upon the subject.

The application I use is the common white paint used for out-door painting, made to the consistence of a poultice by linseed-oil and spread a quarter of an inch thick on coarse linen applied to the injured part. I have always found it give immediate relief, and if applied before vesication has taken place, I have never seen blisters arise after the application.

In one case a slight rigor took place immediately on the cold lead touching the skin, but it went off in a few minutes, and the little sufferer was quite free from pain in ten minutes, though she had been scalded from midway between the umbilicus and pubis to the knees.

Some practitioners to whom I have recommended the lead-poultice have objected to it from fear of absorption of the lead, but I have never seen symptoms of colica pictonum in any of the patients to whom it was applied, nor do I think it likely that a burnt or scalded surface would absorb the lead, as the extravasated fluid in a vesicle remains a long time without being absorbed. I may remark that where vesication was likely to take place, desquamation of the cuticle followed in about eight or ten days after the accident, but the skin was perfectly healthy beneath. I have likewise found it necessary to raise the edge of the poultice when it became dry and hardened, and lay on a little boiled linseed-oil with a feather, but this had seldom to be done more than once or twice. I think this treatment applicable to all kinds of burns, whether there may have been so much heat as to produce merely a red blush upon the surface, in which case the sedative effect of the lead appears to allay the irritation; or where the heat has been so great as to produce vesication, or even disorganization, of the cutaneous texture itself. I am, Sir, yours,

ROBERT S. HANNAY, M.D.

Lurgan Dispensary, Co. Armagh,
Feb. 16th, 1834.

THE LANCET.

London, Saturday, Feb. 22nd, 1834.

THE following brief note has been the means of directing our attention to a subject which deeply interests the Members of the Royal College of Surgeons in London, at the present moment:—

To the Editor of THE LANCET.

Sir,—A word to the wise is enough. It is intended to pull down the houses adjoining the College of Surgeons in Lincoln's Inn Fields, and to enlarge that building to an enormous extent. The outlay will absorb nearly the whole funds of the College. One of the Council of twenty-one was asked in the square of St. Bartholomew's Hospital the other day, how they could think of making such an alteration, now that a Committee of Parliament was sitting with a view to the reconstruction of our medical constitution,—when the councillor ejaculated, with a frown, "—— the Committee."—"Still, if you disregard the authority of the House of Commons, what will the *members of the College* say to such an outlay at this period?"—"—— the members of the College."—Such, Sir, is our treatment. Make what use you please of this information and note, but as the publication of my name would prove my ruin, I must subscribe merely

A MEMBER OF THE COLLEGE.

Although this communication is anonymous, it refers to a matter of extreme importance. Passing over, as unworthy of notice, the alleged imprecation of the man of the Council on the parliamentary Committee and the members of the College, we may state that it is a fact that a project is in agitation for expending between twenty and thirty thousand pounds of the funds of the corporation, in enlarging the building in Lincoln's Inn Fields. This information we have derived from undoubted authority, and it is not less true that the buildings contiguous to the College have already been purchased—one of them from Mr. POLLOCK, the barrister,—with a view to their immediate demolition, that the projected alterations may be effected without delay.

This proceeding is characteristic of that insolent conduct which has signalised the entire career of the Council. During thirty-four years has the building existed in its present form and size. During twenty-six years was the Museum all but closed against the commonalty. During the whole of this latter period was the

Museum utterly destitute of a descriptive catalogue of its contents. During the whole of the like period were the members compelled to enter the institution, on every public occasion, by a filthy back entrance situated in Portugal Street, while the magnificent portals in Lincoln's Inn Fields were reserved for the twenty-one councillors, and their expectant sycophants holding offices in our hospitals;—and during twenty-one years the members of the College resident in London and the suburbs were each taxed by the governing body of this institution in the sum of one guinea annually. Yet of all the monies thus collected, and of all the sums which have been extorted from the candidates for the diploma, no account of their appropriation was ever furnished to the thousands of members constituting the College, until August last, when a printed document was issued by the Council, in which it was stated that the funded property of the College amounted to 66,307*l.* Thus it would appear that this financial paper was issued in mockery and derision of the rights and immunities of the commonalty,—in defiance, too, of the power and authority of Parliament.

The museum has undergone no sudden increase; no bequests of immense stores of books have been conferred on the library; and surely the Council will not allege that the theatre is too small to afford the members accommodation, when visitors, introduced by the Council, generally occupy several entire rows of the benches on every public occasion. Whence, then, originates the present building mania? It is nothing less than a scandalous device of the Council to create a new source of opposition to the institution of a TRIPARTITE FACULTY OF MEDICINE, in which the rights of all legally-qualified medical practitioners should harmonise and blend in one uniform system of medical law. Already is a loud outcry raised respecting "vested interests." Already have the ears of ministers been stunned by the fallacious statements of the monopolists, regarding the vast sums of money which have been expended in collegiate buildings,—the tricksters ever founding on those allegations, the whining, canting question—"Would you like to see those splendid edifices destroyed?"

The Council of the College of Surgeons know full well with what object a parliamentary committee is now sitting to conduct an inquiry into the state of medical practice and education. The Council know that nineteen out of every twenty medical practitioners in the empire, are desirous of ridding themselves of what is useless in the medical corporations, and of beholding all that is advantageous in them united and enforced under ONE FACULTY, — abolishing, at the same time, all baneful and degrading distinctions, and securing the public against the tragical catastrophes which have resulted from the inefficient scrutinies of the examining boards, by compelling all candidates for medical diplomas to undergo a searching, yet fair, practical, examination into their qualifications, in public. Yet, with a full knowledge that such views are entertained by the commonalty of the College, and under the firm persuasion that the House of Commons has sanctioned the appointment of a medical Committee of Inquiry, in approval of those just views,—this Council now dares to hurl the gauntlet of defiance at Parliament, and to offer a direct insult to the many thousand members of the institution, by a proposal to squander the funds of the commonalty,—without once consulting that body,—in altering a building which may be voted by the House of Commons to be little less than a public nuisance. A nuisance it certainly has been to the Commonalty, and but little stretch of argument is required to esta-

blish the fact, that it has been a still worse nuisance to the community, for this intended temple of science has been converted into the den of a band of monopolists. In it schemes for plundering students have been concocted and enforced. In it the most splendid medical museum in the world remained obscured for twenty-six years under the smoke and ashes of the descriptive manuscripts of its illustrious founder.

Concerning this museum the Council should be silent, as the first condition of the trust which consigned to the College the collection of the late JOHN HUNTER, was broken by them annually for upwards of twenty years. The first condition was this:—

"The collection shall be open four hours in the forenoon, two days every week,—*a catalogue* of the preparations and a proper person *to explain it*, being at those times always in the room."

Notwithstanding the condition thus expressed, the museum was destitute of any portion of a catalogue for upwards of twenty-six years. This violation of the trust, therefore, has broken the contract between Parliament and the College, and led to an invalidation of the trust-deed. The preparations, and the room which contains them, have already cost the public no less a sum than thirty-two thousand pounds, and now, after thirty years of possession,—after every condition of the "bond" has been set at nought,—upwards of twenty thousand pounds more of monies *belonging to the commonalty* are to be expended,—with a view,—not to make the museum useful to the profession and the public, but to render it a puppet-show, in which the Council may display their unprofessional pranks, and be gratified by the smiles and adulations of their expectant dependants who are connected with the chartered hospitals.

However, it is to be hoped that this scheme will be considered by some potent authorities to involve a breach of the law, though it is not probable that such a consideration will be induced unless the members of the College interfere to protect their funds from spoliation. At any period it would have been an audacious undertaking on the part of the Council to engage in such a proceeding without holding some previous communication with the commonalty; but, *under existing circumstances*, when a Parliamentary Committee is occupied in inquiring into the state of medical law and the condition of our medical corporations, the project can be regarded as nothing less than a threat of defiance directed against the House of Commons.

AT a recent meeting of the Fellows of the College of Physicians, Dr. SEYMOUR, the man of Sir HENRY, moved that the further consideration of the projected regulations should be taken into consideration that day six months. This was seconded by HALFORD'S particular friend Dr. TURNER, and was lost; upon which it was proposed by Sir GEORGE TUTHILL, a protegé (in Lunacy) of Sir HENRY, that the new regulations should not be discussed until Lady-day next. This was seconded by Dr. SEYMOUR, and carried by a small majority. In a word, the motion for delay may be regarded as a postponement *sine die*. A large portion of the minority evinced their disgust at these proceedings, by declaring that they would attend no more at the College to discuss the regulations.

On the 18th of January, MACLEOD published the substance of the intended new by-laws, and stated, "that the details "might be modified, but that it might be "presumed that the principles contained "in them would be adopted." It was a scheme, it will be recollected, for making all the candidates *Licentiates*, and for

rendering all present and future Licentiates *the slaves* of the existing batch of Fellows. We denounced it accordingly, and,—as happened with some projected changes in the College of Surgeons, a short time back,—a few strokes with the pen dashed the nonsensical rubbish to the winds.

This manœuvre of the College of Physicians had proceeded so far towards completion, that the regulations had been actually printed and circulated amongst the Licentiates. Though MACLEOD,—having alluded to the report of the Committee of Physicians, and stated that he presumed that the principles contained in that report would be adopted, now that he has been directed by his employers to eat his words,—states "that no definite "arrangements have been made, or are "likely to be made at the present mo-"ment." This poor scribe has rather hard task-masters.

INTERCEPTED LETTER.

[ON THE LIBERALITY OF THE COLLEGE OF PHYSICIANS.]

"DEAR DOCTOR SEYMOUR,—I trust, as you perfectly understand that my object has always been to prevent everything like 'change,'—that other term for *revolution*—in our noble and ancient institution, it is now time that you undeceive the Licentiates, and at the next meeting of the College, at once propose that the motion for 'amending our by-laws' be put off *sine die*; or, if you think it would appear more decorous, make the term of postponement, six months, or six years, if you please. I desire, also, that you will, without a moment's delay, get Dr. RODERICK MACLEOD, and Drs. LOCOCK, FERGUSON, and GREGORY, and MACLEOD's other assistants, to set to work, and exert themselves in contriving arguments and bringing forward proofs—real or fictitious—which may exhibit, at this eventful moment, the superior *liberality* of our sacred institution to that of all others in the United Kingdom!

"'Liberality,' my dear friend, is the order of the day, and, mind, you must not only be liberal to *yourself*, but appear to be *ultra-liberal* in the public expression of your sentiments. Now I contend that a spirit of *liberalism* is admirably shown in our College, when compared either with that of the College of Surgeons, or with the 'Rhubarb-Hall Company,' as the Apothecaries Company has been humorously, though not decorously, nicknamed. We give licenses to practise physic to any one who has a degree, no matter *where* he got it, or *how* he got it,—whether he obtained it at Paris, or at the North Pole, or whether he bought it for thirty pounds at Aberdeen, or twenty-five at St. Andrew's, or got it, by a bonus, from the Archbishop of Canterbury. Professional knowledge has, in fact, nothing to do with a degree, and our examinations, compared with the coarse, vulgar, and impertinent interrogatories which are reported of Mr. GUTHRIE, in Lincoln's-Inn-Fields, or the Examiners at Rhubarb Hall, are child's play. If an M.D. can only afford us such answers in Latin as he may have ground into him by a 'private teacher,' our examinators make no difficulty in giving the party a 'license to kill.' Cast your eye over the list of our body for the last twenty years, and see how many of the most successful and money-making physicians obtained their degree of Doctor in order to qualify them to be licensed by us? How, for instance, did Sir WALTER FARQUHAR attain all his wealth and honours? He who lived so rich and died so rich, having, too, actually become confidential physician to the late ever-to-be-lamented and dearly-beloved Sovereign. Why WALTER was nothing else when he first practised in London than a subordinate. He afterwards bought a thirty-pound degree at Aberdeen, which instantly admitted him into our College, and *instinctively* made him a 'learned physician.' My dear friend, the never-to-be-forgotten WILLIAM HUNTER became also a 'learned physician,' entirely by the *liberality* of our College. He was a poor Scotch surgeon (for those barbarians have no subordinates, as Mr. BRODIE calls them), and, after being some time settled in London, saw the advantages which a doctor's degree would afford him. He accordingly made a purchase at Glasgow, which, by the *liberality* of our College, at once procured him a 'license.' Our *liberality*

was also most conspicuous towards the late Dr. BABINGTON. I remember him the humble apothecary of *Guy's Hospital*, and for the small sum of twenty-five or thirty pounds (I forget which) he obtained a Scotch degree, which at once entitled him to our 'license.' And what more did we not do for him? Such were his respectability and kind-heartedness, that a few years before his death we presented him with a 'fellowship.' This conduct I say was *ultra*-liberal. Then, only look at the College list of the present hour, and I think you will see names in it which have no title to the high distinction of a license, and the owners of which would never have been admitted to practise, had not *liberality* been the maxim of our College! What was it that placed Sir CHARLES CLARKE in the high position which he now occupies? Nothing but my ultra-liberalism. I remember him a surgeon's mate in the navy, wearing his brother JOHN'S old breeches,—for JOHN was parsimonious, and lived, for economy's sake, in a very humble way with his mother, who then kept the 'wax works' in the Strand. Having acquired great practice as a *man*-midwife, and published that work which he so unblushingly entitled a work on 'Female Discharges,' but of which, Heaven be praised, I see he has now changed the title, nothing remained but to get our 'license.' I accordingly obtained for him a degree of 'Doctor in Medicine' from the Right Reverend Father in God, the ARCHBISHOP of CANTERBURY, which, considering the *peculiar* nature of CHARLES'S avocations, came more appropriately from that high church dignitary than if he had purchased it at Aberdeen or St. Andrew's. Besides, the circumstance has entitled him to the appellation he has now universally acquired from the ladies of the 'Divine Doctor.' Some may talk of the impropriety of churchmen having the power to grant medical degrees, but I must confess that, as a general principle, the objection is perfectly absurd. Yet in the case of the 'Divine Doctor,' the new creation, as Sir ASTLEY humorously observed, was well *conceived*, and I was proud that my efforts to accomplish so praiseworthy an object were not *abortive*.

"The College list affords endless other proofs of the *liberality* of its ruling powers in licensing midwives, such as LOCOCK, FERGUSON, and others, all of whom have emanated as 'learned Doctors' from the north, and have, by the unbounded *liberality* of our College, been admitted 'Licentiates.'

"But I take not to myself any credit for such additions; on the contrary, I contend that these are instances of a most mistaken *liberality*. What can be more inconsistent than for the Royal College of Physicians of London, to admit within their hall a host of *midwives*! Is it not humiliating to permit men to form an integral part of our College, — whose practice consists in that very department of surgery which Mr. BRODIE and others consider beneath the dignity of even the *pure surgeon*? Yet such has been our *liberality*, that, excepting one or two men of undoubted talent, such as the late Dr. GOOD and Dr. ARMSTRONG, I do not recollect one whom we ever rejected!

"And pray, my dear Sir, look to the 'Fellowships,' look to the tag-rag-and-bob-tail Fellows, for whom our *liberal* principles have been the means of gaining admission into the College! *You* know, full well, how that prince of Aberdeen doctors, Sir JAMES MACGREGOR, became a Fellow, and I was also over-persuaded when I admitted HOLLAND. It certainly was not the scientific acquirements of these persons and others which entitled them to so high a distinction, and I am decidedly of opinion, that in order to preserve the dignity, integrity, legitimacy, and, above all, the *emoluments* of the profession, it becomes the bounden duty of high-minded, straightforward, honourable, and virtuous Fellows, to be extremely cautious and jealous on whom they bestow the highest honours of the Royal College. I am myself determined to resist the admission of a parcel of low, intriguing aspirants, who are constantly tormenting me on the subject, and endeavouring, by all kinds of manœuvres, to obtain my acquiescence in their elevation.

"I conclude, with requesting that you will endeavour to influence the press in all quarters on this important matter, and request the Fellows to point out to such friends as they may have among the members of either House of Parliament, that the Royal College of Physicians has acted, and continues to act, *more liberally* than any of the other medical corporations of this metropolis,—that they are far less selfish, as is incontrovertibly proved by comparing their well-known poverty with the riches of the College of Surgeons,—and that when the axe of reform comes to be applied, they will find very little to cut away in our most venerable institution!

"Believe me, with unfeigned truth and unalterable friendship, yours,

"— ——."

PHRENOLOGICAL SOCIETY.

Session 1833-34.

Dr. ELLIOTSON, President.

OBJECTIONS OF DR. KIDD TO PHRENOLOGY.—REMARKS THEREON BY DR. ELLIOTSON.

THE PRESIDENT, the other evening, on taking the chair, drew the attention of the members to some remarks on the doctrines of phrenology, which had been made public by opponents of the science. He remarked to the Society, that it was their duty not only to cultivate their science and watch its progress, but to consider whatever was advanced against it. During the recess, he had met with two works in which phrenology was opposed, although in one of them the admissions made at the same time in its favour were such in number and weight as would not have been made a few years ago, and such as proved the steady progress of "their success." This was a "Bridgewater Treatise," the production of a highly informed and excellent man, an Oxford professor, Dr. Kidd. The author admitted that "it might be affirmed with truth, that, considered as an abstract philosophical speculation, phrenology is highly ingenious, and founded upon *unobjectionable principles*; and that while the general conclusion is *inevitable* with respect to the collective functions of the brain, there is *nothing unreasonable* in supposing that *specific parts serve specific purposes*." Dr. Kidd declares that it "cannot be doubted, indeed, that it is a matter of fact which is constantly open to our observation, that the degree of approximation in the structure of the brain of *other* animals to that of man, bears a very obvious relation to the degree of intelligence manifested by the various classes of animals."—"Nor does there appear," he says, "on the ground either of reason or of religion, anything objectionable or absurd, in the assumption antecedently made, that the intellectual and moral tendencies of the one may, in a qualified sense, be determined, or at least modified, by the peculiar structure of the body. That they are frequently coincident with certain peculiarities of corporal structure, is a matter of actual observation." He asks, "Is it absurd to suppose that the brain, being a very complicated organ, made up of distinctly different parts, those parts are subservient to the exercise of different functions? Or, since it is evident that in every other individual organ of the body, where there is an identity of structure, there is also an identity of function in all the parts, may we not fairly presume that, were the integral parts different, the effects produced would be different; and, consequently, that as the integral parts of the brain differ from each other, the offices of those parts may be different? Or, again, will it be denied as a matter of fact, that different faculties and propensities manifest themselves in different individuals; and is it unreasonable, on the ground of analogy, one only ground in this case, to suppose that they manifest themselves through the agency of different instruments? And since the visceral nerves are appropriated to the mere vital function of nutrition, and the spinal nerves to general muscular motion and common sensation, and the nerves of the special senses occupy but a very small portion of the brain,—to what assignable purpose can the great mass of the brain be applied, if not to the operations of that intellectual and moral principle, which, after the obstruction of the organs of nutrition, motion, and sensation, is the only imaginable part of our present nature?" Dr. Kidd lastly remarks, "1st, That just as the young of other animals have a comparatively higher degree of intellect at birth than the human infant, and no gradual development of faculties takes place, while the human faculties are slowly unfolded, so in the inferior animals all the parts of the brain are in as perfect a state at the moment of birth, as they are in the adult animal of the same species; while in the human species, even according to Wenzel, although the brain makes continual and rapid advances to its ultimate magnitude and perfect state, from the period of conception to the seventh year after birth, yet all the parts have not obtained their full size till the age of seven, and, during the evolution of the parts peculiar to the human brain, the peculiar faculties of the human intellect are proportionably developed; and, finally, that until *those parts* are developed, *those faculties* are not clearly perceptible."

The President here congratulated the Society on the circumstance, that while the truth of phrenology had really been admitted by the writer in the *Journal Hebdomadaire*, whose article was examined by Mr. Drew at a previous meeting, a professor of Oxford,—the university of all that is established and orthodox,—should admit the soundness of every one of its principles; but he lamented that Dr. Kidd, having shown so much soundness of judgment and freedom from intellectual and moral prejudice, should all at once have deviated from his philosophical and manly course. "The rock on which Dr.

Gall and his implicit advocates have split," says he, "is the attempt to fix the local boundaries of the several faculties of the soul. Had he satisfied himself with developing the structure of the brain in the various classes of animals; and had he been content to show, that in tracing its structure from those animals which manifest the least indication of intelligence to those which exhibit it still stronger and stronger, as it proportionably advances to the structure of the human body; and, lastly, had he only drawn from the premises the probable conclusions, that specific parts had specific uses with respect to the manifestations of the immaterial principle of animal existence,—and assuredly brutes are endowed with such a principle (though as being devoid of moral sense, they are not fitted for a future state, and, consequently, perish when their bodies die),—had Dr. Gall been content to stop at this point, without wanting to define the local habitations of the supposed specific organs, he would have acquired the unalloyed fame of having developed a beautiful train of inductive reasoning in one of the most interesting provinces of speculative philosophy, whereas in the extent to which he has carried his views, his doctrine has become ridiculous as a system."

Now, observed the President, one would imagine that Dr. Kidd had ascertained by observation that Gall's almost endless facts in proof of the existence of each organ were no facts at all. Yet, on the contrary, without offering a single disproof of a single organ, Dr. Kidd barely and boldly asserts, that all Gall's facts amount to no facts, for such is the assertion that Gall has split upon the attempt to fix the local boundaries of the faculties. Phrenologists who have perseveringly examined not only Gall's illustrations and proofs, but head after head that Gall never saw, and found his assertions and observations true to nature, must laugh at such a professorial sweep.

Besides, this doctrine of particular portions of the brain serving for particular faculties, said by Dr. Kidd to be "highly ingenious, and founded upon unobjectionable principles," and to have "nothing unreasonable" in it, was suggested to Gall by the observation that persons mentally and morally remarkable in a certain way, had a striking size of a particular part of the head. So far from splitting on this rock, Gall built his house upon it, while most metaphysicians build theirs upon sand; and when the floods of objection came, and the rain of abuse fell, it remained stable! Not only the whole system sprung from this observation of relation, but even the beautiful grouping of the organs was its result, and not anticipated by Gall. The situation of the organs of the lower propensities, and the inferior mental faculties, common to us and brutes, at the lower part of the head, and the superposition of those of the higher, one above another, according to their excellence; the situation of the intellectual, in front of the head—the propensities at the posterior and lateral parts,—and of the moral sentiments at the superior and middle parts,—facts assented to now by most persons who once would not admit the doctrines of phrenology at all—not even that the brain was the organ of the mind—was a discovery which resulted from the successive and detached discovery of particular organs. When Gall had discovered organ after organ, he found them arranged in this beautiful manner; and when we survey this, and the various developments of those respective regions in the different tribes of animals up to man, and in different races and individuals of the human kind, our admiration and wonder at the plan and beautiful order of the Creator is unbounded. No human ingenuity could have invented so beautiful a system.

To show how carelessly Dr. Kidd had studied the science, the foundations of which he attacks, while he admires the structure, the President read the following passage:—

"But there is an occasional absurdity in the application of his theory, which, though obvious, does not seem to have been noticed. Let us suppose, for instance, the case of a *murderer*, and that a disciple of Gall were to maintain, that as the crime of murder proceeds from the operation of the organ of *destructiveness*, that organ would be found highly developed in such an individual; and yet, upon actual inspection, this was found not to be the case! Here, although the disciple of Gall might be disappointed in finding no such development, a plain reasoner would not be so disappointed; for, is it not obvious that avarice, or shame, or jealousy, might, in a moment, operate so powerfully as to lead an individual to the crime of murder, whose nature and habits were as far as possible removed from the propensity to that crime, and who, consequently, according to Dr. Gall's own principles, would be devoid of any undue development of the organ of *murder*?"

Now (observed Dr. Elliotson) this proves Dr. Kidd, and I say it most respectfully, to be unacquainted with the writings of phrenologists. He should have known, that though the faculty of *destructiveness*, occasioning violence and fierceness of dis-

position, is generally pronounced to be large in murderers, yet that phrenologists equally maintain that the heads of all murderers are not alike – that in some the organ of *destructiveness* is of but ordinary development, and the murder is to be accounted for by insanity, by a deficient development of the organ of *benevolence* and the high moral feelings, and an excess of the organ of *self-esteem*, love of notoriety, love of property, &c. Some murders are committed by poison, some by stratagem, some with ferocity. Phrenologists know all this, and speak of these matters like "plain reasoners;" and whenever they have asserted that the organ of *destructiveness*, in a particular instance, was not of an inordinate size, have acted as Dr. Kidd conceives plain reasoners ought, they have been exclaimed against and ridiculed, and told that they made heads to square with their doctrines, to get out of their difficulties.

Dr. Kidd candidly acknowledges that with respect to ourselves, the study of the system may be attended with the happiest consequences; for if, from the contemplation of it, we can be strengthened in our conviction of the fact which both reason and revelation teach us,—that each individual is liable to particular temptations, depending on his specific temperament,—we shall then have an additional memento of our frailty, an additional incentive to watch over and combat the "sin which doth so easily beset us." The President observed that there was more advantage in it than this, as he had pointed out in his notes to Blumenbach's physiology; that when a person was receiving praises for his talents or his virtues, he must from phrenology have an additional motive for humility. He must see that they depend upon his happy organization, and not upon himself, for the Creator has made us, and not we ourselves.

Dr. Kidd, at the same that he is so candid, falls into the inconsistency, and even contradiction, of saying, that phrenology, "in its individual applications, is not only useless, but of a positively mischievous tendency, for it is evidently more safe to judge of others by their words and actions and the general tenor of their conduct, than to run the risk of condemning an individual from the indication of some organ, the activity of which, for a moment allowing its existence, may have been subdued by the operation of moral and religious motives." Now, replied Dr. Elliotson, if it were useful to ourselves to observe our own natural organization, it would be almost impious to fancy that the work of the Creator in the organization of heads which are not on our own shoulders must lead to mischief. It is irreligious to believe that the knowledge of any natural fact can be otherwise than useful. He begged to quote his own words from page 214 of his Physiology. "If we detect the sign of great talents and virtues in the heads of others, we may love them the more, as superior and highly-favoured beings; whereas if we detect the signs of great virtues and talents in our own heads, we may learn to take no praise to ourselves, but be thankful for the gift; and if we detect the signs of intellectual deficiency and vice in others, we may learn to pity rather than to censure. We may learn not to judge others, not even our own selves, but to leave judgment to Him who only knows exactly what strength of evil inclination, what weakness of good, and what unhappy external circumstances, we have had to contend with. In truth, phrenologists never judge of actions. They know that character depends on two things,—internal and external circumstances,—that the stronger the peculiar internal organization, the weaker will be the effect of external circumstances; and the less decided the organization, the more powerful will be the effect of external circumstances." A very decided organization acts much, and forces on the person, in spite of external circumstances.

The other work to which the President called the attention of the Society, was one written by a curate of the name of Taylor, and which, he said, he should never have heard of, but for the advertisement, in which he saw that young persons were to be put on their guard *against the wicked doctrines of phrenologists as well as those as those of geologists!* The learned Doctor's commentary on this production must form the subject of a report in our Journal next week.

DISTRICT DISPENSARIES.

To the Editor of THE LANCET.

SIR,—If you see no objection to the following suggestions, I presume they are sure of insertion, and will thus afford an opportunity for the profession to offer a judgment thereon. It were useless to relate examples in proof of the distressed condition of the larger portion of the medical practitioners in this metropolis. The facts are notorious, and the causes nearly as well known. Among *these*, too little stress is laid on the abuse of medical charitable institutions. As this communication may perhaps obtain the notice of your correspondents, I will at present

forbear a minutely particular exposition of the plan offered for their consideration, seeing that it may be more convenient to do so in the form of answering objections, should such be raised to the principle or the details with which you are now furnished. It is an established truth that dispensaries are injurious to the profession, without being beneficial to the poor; often, indeed, the apothecary being the only object supported and relieved by the institution. To remedy this, let a certain number of general practitioners in every district associate themselves under well-digested and practicable regulations, as the officers of "district dispensaries;" one, two, or more "taking in patients" daily. Let there be no distinction in the designation and duties of the officers. Let them use their influence with their patients and connexions, to obtain small subscriptions for the support of a dispensary, house, or rooms, a dispenser, and drugs. Every subscriber of a guinea annually to be entitled to have *one* patient always on the books. Every poorer subscriber of *5s.* per quarter to be entitled to have the same privilege, and also available in his own favour, or of any member of his family. The expenses of such establishments would be inconsiderable under medical control. They would not rob the funds of 130*l.* a year for the dispenser's salary, and his duty should be confined exclusively to cupping, bleeding, and compounding medicines. The surplus funds should be divided among the medical men.

I am, Sir, yours, &c.,

A GENERAL PRACTITIONER.

LONDON HOSPITAL.

PUNCTURE OF THE HEART.

GEORGE ASHTON, ætat. 17, was brought into Mellish's Ward on the evening of October 3, having received a stab in the left breast. From some of his friends, who had assisted in bringing him to the hospital, it was ascertained that the youth had been insulted by a sweep-boy, and had resented the offence by a blow, when the sweep thrust the blade of a small pocket-knife into the breast of the boy, which brought him immediately to the ground. He was received into the hospital *twenty minutes* after the wound was inflicted, in a state of faintness; the countenance anxious, and very pale; the pulse scarcely perceptible. Very soon after admission, and while lying on the shutter on which he was brought in, he expressed a desire to be taken to the water-closet, but the request could not be complied with, from his extreme faintness. He was put to bed, a small pledget of lint being placed over the external wound. A fit of vomiting almost immediately followed, and, after a few gasps, he expired.

Post-mortem.—The external wound was about an inch and a half in extent, situated three inches above, and to the right of, the left nipple. The knife entered the chest an inch from the sternum, and passing obliquely downwards, between the third and fourth ribs, went completely through the right, and nearly into the left, ventricle. The delicate lining membrane of the left ventricle prevented the communication between the two cavities, as the muscular structure of the septum was completely divided. The instrument entered the right ventricle, just below the origin of the pulmonary artery. The opening was about one-third of an inch in length. The pericardium contained twelve ounces of blood; the cavity of the chest rather more than three times the quantity.

HOPITAL DES ENFANS MALADES, PARIS.

ANASARCA AFTER SCARLATINA.

THE following is the autopsy of the case of anasarca supervening on scarlatina, observed and reported by our Paris correspondent, which was inserted in No. 543, page 694. The body was examined the day succeeding the death.

Cavity of Abdomen.—The abdomen contained a small quantity of greenish-coloured serum. The lining membrane of the stomach presented a light rosy injection, but was of the natural consistence, and healthy; spleen small and solid. The kidneys were much injected, of a dark purple colour, and, when examined as to their internal structure, appeared healthy.

Cavity of Chest.—The costal surface of the left pleura was rough, granulated, and adherent so strongly to the lung as to detach a portion of the latter when it was removed from the chest. The surface of the pulmonary serous membrane was covered with a thick layer of pus, and the cavity of the left pleura contained about five ounces of that fluid. The false membrane uniting the lung to the parietes of the thorax was very strong, and evidently of no very recent formation. The left lung also contained a pretty large abscess, lined with a false membrane, and filled with thick pus; it also contained various solid tubercles, and the bronchial glands were all converted into that matter. The

œsophagus and trachea were healthy. The greater part of the right lung was healthy and crepitant, but near the summit we remarked a few points of lobulated pneumonia.

The mucous membrane of the great intestines was of the natural colour, but much thickened, and covered with numerous small granulations and patches of false membrane. The upper portion of the large intestine near the arch of the colon, was found in a state of white ramollissement, without any trace of inflammation.

ST. BARTHOLOMEW'S HOSPITAL.

To the Editor of THE LANCET.

SIR,—Much interest has recently been excited respecting a case of dislocation treated at St. Bartholomew's Hospital. I refer to a dislocation of the femur into the ischiatic notch, admitted under the care of Mr. Lawrence, in which 115 ounces of blood were abstracted at one bleeding, and an aggregate of half a dram of tartar emetic administered. The dislocation was reduced at the expiration of two hours, but the man subsequently died.

The case is additionally interesting, as Mr. Lawrence is reported to have said the man died from phlebitis. If you can supply a minute report, including the post-mortem examination, you will oblige many. I am, Sir, your obedient servant,

A WEST-END PRACTITIONER.

Feb. 15, 1834.

MR. DERMOTT.

To the Editor of THE LANCET.

SIR,—The *Westminster Medical Society* has never shown itself particularly forward in advocating the real interests of the profession, although professedly instituted "for the advancement of medical science." (See the title-page of the laws of that institution.) The expulsion of Mr. Lambert, for merely doing his duty in *speaking the truth*, is still fresh in the memory of thousands, and the vote of the Gregorians on the 8th reform resolution will not be speedily forgotten. Equally unjust, Sir, is the treatment Mr. Dermott received on a late occasion at the hands of this Society; having been blackballed, in fact, by the Committee. At the solicitation of his friends Mr. Dermott has consented to be proposed, as the laws permit, a second time, and he was so proposed on Saturday last, and will be balloted for on *Saturday the 22nd*. Your Journal will, by that time, have been seen by hundreds, and I trust all who see it will (if they be members) flock to Windmill Street, and prevent the Society from the disgrace of refusing the admission into it of a man of great talent and profound anatomical knowledge, because HIS POLITICS (for that is the secret of the affair) differ from those of the Committee and some few of the "dons." Let me caution the members to take care that the ballot-box *is passed round to every member*, as it has long been the custom merely to hand it to those sitting in the front rows. This is all very well where no opposition is expected, but, in the present instance, where bad feeling towards the gentleman proposed is known to exist, it might be the means of securing a triumph to a party, which, in fact, I firmly believe, forms a very small portion of the Society. In conclusion, allow me to say that this letter is written without the sanction or knowledge of Mr. Dermott, and if this be doubted, I shall be ready, at any time, to give you (in confidence) my name and address, and proof of what I say. I am, Sir,

Your constant reader,

A MEMBER OF THE WESTMINSTER MEDICAL SOCIETY.

NOTE FROM DR. FERGUSSON.—LITHOTRITY.—*To the Editor of* THE LANCET.—Sir, When speaking a short time since at the *Westminster Medical Society* on a case of lithotrity, I expressed my especial pleasure at the absence of all preparations for the operation,—alluding to those appalling ones attendant on lithotomy,—and not to the "bed and vice" (words not used by me) employed by some lithotritists. This brief explanation appears only due, as otherwise it would seem, from the report in THE LANCET, I had made a comparison and given a preference, my incompetency to do which naturally arises from the fact of my having only witnessed the one method in the hands of Mr. Costello, by whose invitation I felt much honoured. His judgment, skill, and dexterity, and very kind and benevolent attentions to his poor patients, at once command our admiration and esteem. I am, Sir, your most obedient servant,

P. BALLANTINE FERGUSSON.

London, Feb. 17, 1834.

VINDICATION OF THE STATEMENTS OF "A BARTHOLOMEW PUPIL."—*To the Editor of* THE LANCET.—Sir, My friend Mr. ——, having gone into the country, and as he was the writer of the letter complaining of the conduct of the dresser, Mr. Coates, I feel myself called upon, by the arbitrary and insolent letters of the three students, which appeared in the last number of your journal, to bear testimony to the accuracy of his statement. I positively affirm that Mr. Coates's position prevented a great many pupils from seeing the operation, and that he might have remedied it if he had chosen so to do. The repeated exclamations which those gentlemen

pretend not to have heard, were audible enough to at least twenty of the students with whom I have conferred on the subject; in fact they seem to have verified the proverb, "There are none so deaf as those who won't hear." If, therefore, these students will favour me with an interview in the museum (where I shall be at 12 o'clock on Tuesday morning, the 23th inst.), I hope an explanation will take place, otherwise the castigation alluded to by one of them shall fall heavier than the writer had an idea of. I have the honour to be, Sir, your obedient servant,

Queen-street, Long Acre. J. MASON.

UNIVERSITY OF LONDON—SUBJECTS.—*To the Editor of* THE LANCET.—Sir, In the last LANCET, there is a note purporting to be from three students of the medical school in this institution. It expresses the writer's sympathy with a Bartholomew Student who complained of a scarcity of subjects for dissection,—states that the scarcity is general among the schools,—alleges that only one body had been brought in for examination during a week, and invites you, Sir, to address a notice on the subject to the demonstrator. It is undeniable (as indeed your correspondents allow) that the schools have not been sufficiently supplied during this session. This, as I know, has arisen from a cause over which there is no control, and the nature of which is easily ascertained by reference to any surgeon in extensive practice among the poor, especially those connected with the parishes. The scarcity is not confined to London, I am informed that it extends more or less even to Paris. Having made inquiry in different quarters, I have reason to know that neither we nor our brethren of St. Bartholomew's have any particular reason to complain.—Your correspondent must have taken a very unfortunate week for observation of the number of bodies received, for to my knowledge, *at least* THREE were given to the students during the week immediately preceding the date of his letter.—As to the request that you should notice the matter to the demonstrator, I, as an old student, would inform my fellow-labourer, that the Council (who in their absence are represented by the Secretary) are the body to be applied to, if necessary—they, and not the demonstrator, being responsible to us. I am sure you will not mistake me, Sir, when I add, that in place of addressing you in the first instance, or publishing a letter, it might have been as well,—it certainly would have been more in unison with the spirit of kindliness and cordiality existing among all here,—if explanation had been asked, either from the Dean of the Faculty, or the Secretary, or the Demonstrator, after which an appeal to you, if considered useful, would, in my judgment, have been better timed. I have the honour to be, Sir, your obedient servant,

A MEDICAL PUPIL, U. L.

University of London, Feb. 17th.

CORRESPONDENTS.

The advice contained in the latter part of Mr. *Hardy's* letter was excellent. Its adoption would fully provide for the exigency stated in the former portion, and the specific proposal, therefore, with which it was accompanied, does not need promulgation in our pages. Mr. H.'s exertions with the member of Parliament for his division of the county, deserve the highest praise, and we can inform him that our recommendations on the subject have been generally and most successfully adopted.

What would be gained by the publication, at this time, of the letter signed *An Obstetrician*? The appointment, of course, was arranged in London, —not in Glasgow. We cannot insert the letter from Glasgow signed *An Accoucheur*, unless the author will favour us with his name confidentially.

We are obliged to *An Assistant Surgeon* for his proposal, but cannot accede to his request.

The second communication of *Dr. O'Beirne* has been received, but it is out of our power at present to say when we shall be able to insert it.

We cannot notice the regulation respecting the admission of strangers to the dissecting-room of the L. U. It is most probably a very proper one.

The trial mentioned in the note signed *A Student of King's College*, will occur too late for notice this week, and may not possess sufficient general interest for notice on another occasion.

***A Sincere Friend.*—It is to be hoped** that provision will be made for securing each practitioner the proper title in the first instance.

***A Subscriber.*—He would be liable to** the penalties. The question proposed evidently implies the virtual supply of medicines by the prescriber. The druggist would be as much his servant as a person acting in his own surgery.

***A Bartholomew Pupil* must favour us** with his name, confidentially. There would be an end to all fairness in literary controversy if anonymous writers were allowed to contradict the statements of parties who think it right to openly authenticate their communications.

The parallel drawn by *E. V. M.* between the effects of the suspected gas and the symptoms of the acknowledged disease, is in many respects correct, and the speculations are ingenious; but speculations on the subject have hitherto produced no good result, and we do not think those of our correspondent likely to prove an exception. One strong and unquestionable fact in support of the consumption of a sufficient quantity to produce by union with oxygen some gallons of fluid,—would entitle the paper to publicity.

The paper on the Atomic Theory has been received, and shall be subjected, at an early period, to a careful examination. A cursory glance enables us to observe that the labour which has been bestowed on the subject entitles the author to no small share of commendation.

The receipt of the case forwarded by our correspondent at Winslow was not acknowledged, because the case was marked for immediate insertion, which accident afterwards prevented. It will appear in an early number.—The letter signed *A. B.* will be published.—The cases sent by Mr. *Wetherfield*, and Messrs. *Thompson* and *Baller*, and also the letter of Dr. *F. T.*—shall appear. —*M. G.*, of Kendal, should authenticate his case by the addition of his name.—Mr. *F. W.*'s packet has been received.—The communications of *Philalethes*, Mr. *N. J. Cooper*, and Mr. *C. Clark*, have been received.

We have received Mr. Whatton's "Address," &c., Mr. Atkinson's "Medical Bibliography," Mr. Eccles's observations on the "Ulcerative Process," Dr. Wilson Philip on "Minute Doses of Mercury," and Mr. Tuson on "Hernia."

We entertain many objections to the proposals contained in the letter on "district dispensaries" inserted at page 837, but have not room this week to state them.